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VOL. VIII. ANN ARBOR, MICH., JULY, 1891. No. 3.

ORIGINAL ARTICLES.

FRANKLIN'S KITE EXPERIMENT.

BY ALEXANDER MCADIE, M. A.

Picture to yourself, if you will, an infinite ocean of jelly, in which in various parts, a liquid is entrapped and entangled. This liquid will remain fast bound there until the walls of the jelly which imprison it, in some way yield or are ruptured. This will give you, as one of the great physicists of our day suggests in his "Modern Views of Electricity," a fair model of the general insulating atmosphere. Made up, as it is, of a mechanical (not chemical) mixture of oxygen, nitrogen, water-vapor, traces of carbonic acid, carburetted and sulphuretted hydrogen gases, ammonia, nitric acid, and (though last, by no means least) dust, our atmosphere will stand for the jelly,—an infinite ocean of it,—and the entrapped liquid, the electrification, (or electricity, if you will), which we find on the rubbed surfaces of glass, resin, etc., or in a natural way upon the surfaces of the little vesicles which in aggregate make up a cloud. While the jelly-walls hold, the entrapped fluid is like gas in an untapped gas well. That is, while the air completely surrounds the body on which an electrical charge resides, the charge will remain there, fast bound. Let this air become very damp, or filled with dust, or both, and it is analogous to having the jelly become itself very much of a liquid and its restraining power, therefore, small; but let the general atmosphere be fairly pure and dry, and there is not much difficulty in "insulating" and "storing" your electricity.

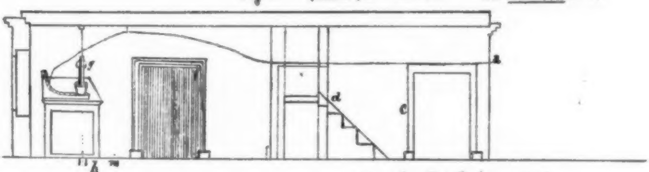
What we will want to particularly consider, in any investigation of the electricity of the air, is the "dielectric strength" of the air; or in the familiar language of our analogy, the pressure which the confined fluid will exert, and the strain which the ethereal jelly can withstand before the liquid breaks down the confining walls and escapes with a greater or less degree of violence. This is what we are now attempting to do, viz.: get at the electric stress in the air, and it may not be long before we will be able to determine by measurement, whether or not, a thunder cloud overhead, has an electrification sufficiently strong to break through the air barrier between it and us; and whether we shall have lightning from that cloud; or whether pushed and drawn by the winds, that cloud-mass with its charge will harmlessly pass by, seeking elsewhere opportunity and place to discharge or lose its electrical energy.

That unique genius and great philosopher, who, as a militia Colonel, underwent the rare experience of having his pet pieces of electrical apparatus shaken from off their shelves by the ragged and ill-timed firing of a salute in his honor, by the regiment he commanded,—(which, by the way, was about the only service of a war-like character that regiment ever performed,—it being disbanded by order of the King soon after its organization), was the first, as we all know, to bring to the attention of the scientific world, the general identity of lightning with the electricity developed by the large frictional machines of that time. The famous kite experiment is described by Franklin in a letter dated October 19, 1752. (Letter XII., Franklin's Experiments on Electricity). "Make a small cross of light sticks of cedar, the arms so long as to reach to the four corners of a large thin silk handkerchief when extended. Tie the corners of the handkerchief to the extremities of the cross, so you have the body of a kite which being properly accomodated with a tail, loop and string, will rise in the air, like those made of paper, but being made of silk is better to bear the wet and wind of a thunder gust without tearing. To the top of the upright stick of the cross is to be fixed a very sharp-pointed wire rising a foot or more above the wood. To the end of the twine, next the hand is to be tied a silk ribbon, and where the silk and twine join, a key may be fastened. This kite is to be raised when a thunder gust appears to be coming on, and the person who holds the string must stand within a door or window, or under some cover so that the silk ribbon may not be wet, and care must be taken that the twine does not

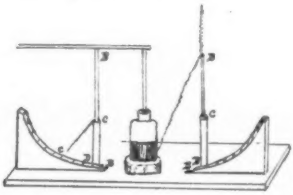
touch the frame of the door or window. As soon as the thunder clouds come over the kite, the pointed wire will draw the electric fire from them, and the kite with all the twine will be electrified and the loose filaments of the twine will stand out every way and be attracted by an approaching finger. And when the rain has wet the kite and twine, you will find the electric fire stream out plentifully from the key on the approach of your knuckle."

There is a certain audacity shown in this experiment which fascinates, and undoubtedly had a good deal to do with the deep interest which this experiment excited when first announced, and still excites. It was audacious. The fate of Richman was like a clap of retributive justice. We may not here go into the details of

Fig. I. reproduced from "Phil. Trans." Richman's Room.



h = where Prof. Richman stood.
g = " his head was.
m = " Mr. Sokolow (the engraver) stood uninjured.
c = a bit of the door carried to d by the lightning.



Richman's Electrometer.

Fig. 2.

that accident, but the rough diagram of the room in which the St. Petersburg experimenter was killed, as published that year, 1753, in the "Philosophical Transactions" is worth reproducing. (Fig. I and Fig. II). And, supposing that we did not know from direct evidence that it was lightning that killed Richman, an expert of to-day, noticing the escape of a companion standing at the time within three feet of the victim, would recognize by this, and the peculiar "tearing-off" and "throwing" effects, the existence of an electrical current of exceedingly high potential, small amperage, and oscillatory in character. In order to have before us at a glance, the experimentation of the fifty years following Franklin's work, I have made this brief table of the chief experimenters and their results:

DATE	NAME.	EXPERIMENTS.	REFERENCES.
1752	Kinnersley, Boston......	Observations on the electrification of the air.	"Franklin's Letters," "Phil. Trans., 1763, 1773."
1752	LeMonnier, Paris........	Observations on the electrification of the air.	"Mem. de Paris," 1752.
1752	De Romas, Paris........	Observations on the electrification of the air.	"Mem. Sav. Etrange," II. 1755.
1752	Abbe Mazeas, Paris......	Kite experiment, independently of Franklin.	"Phil. Trans ," 1753.
1752	Abbe de Nollet, Paris....	Speculations on the theory of electricity.	"Recher. sur les causes," etc., Paris, 1749-54. "Recueil des letters, etc.," Paris, 1753. "Letters sur l'elec," Paris, 1774.
1752	Watson, London.........	Experiments on electrification of clouds and protection from lightning.	"Phil. Trans.," 1751, 1752.
1753	Richman, St. Petersburg	Electrical gnomon. Killed by lightning, Aug. 6 1753.	"Phil. Trans.," 1753.
1753	Canton, London..........	Observations of the electrification of clouds.	"Franklin's Letters," and "Phil. Trans.," 1753.
	Collinson, London.......	Franklin's correspondent who introduced F's experiments to the notice of the Royal society.	
1753	Pere G. B. Becaria, Turin.	Systematic observations with an electroscope.	"Lettere del Elettric," Bologna, 1758.
1754	D'Alebard, Paris.........	Experiments and observations during thunderstorms.	"Mem. l'Acad. r. des Sciences," May, 1762. "Hist. abregu d'ebe," 1776.
1769	Cotte, Paris...............	Memoirs on Meteorology.	"Mem. Paris," 1769-72. "Journ. Phys.," XXIII, 1783
1772	Ronayne, London........	Observations with regard to fog and mist.	"Phil. Trans.," 1772.
1772	Henley, London..........	Observations and quadrant electrometer.	"Phil. Trans.," 1772.
1775	Cavallo, near London....	Improvements in apparatus and observations on fogs, snow-clouds and rain.	"Treatise on Elec.," 1777.
1784	De Saussure.............	Improvements in apparatus and observations.	"Voyage dans les Alps."
1786-7	Abbe Mann...............	Daily observations with an electrical machine of a number of revolutions required to produce a given spark—with a record of the meteorological conditions.	
1788	Volta......................	New electroscope and burning match.	"Lettere Sulla Meteor.," 1783.
	Crosse......................	Experiments with collectors.	"Gelb. Ann. Bd.," 41 S. 60.
1791	Read........................	Improved insulation and conductors.	"Phil. Trans.," 1791.
1792	Von Heller, Fulda......	Observations.	"Green's J. d. P.," Bd. 4
	Schubler, Tubingen.....	Observations using "weather-rod" method.	"J. de Phys.," LXXXIII, 184.

We repeated Franklin's kite experiment, one hundred and thirty-three years after its first trial, at the Blue Hill Observatory, some ten miles out from Boston. The summit of the Hill has an elevation of 635 feet above sea-level, and is therefore the highest point on that section of the Atlantic sea-board. With the exception of two or three neighboring hills, all the surrounding country is low and level. The average elevation is under 100 feet, and on all sides this lowland is well watered, having many ponds and small rivers. An exact statement of the apparatus used, may be found in a paper in the "Proceedings of the American Academy of Arts and Sciences," and it will be enough here, to say, that we used a Multiple Quadrant Electrometer, a portable dry battery of 100 Beetz cells, set up in series, a newly set-up Daniel cell, two large kites, silk-covered and tin-foiled on the front face, 1,500 feet of strong hemp fish-line, around which in a close spiral was wound No. 22 uncovered copper wire, some insulating materials, an electrometer commutator, binding screws, a condenser, etc. After all, about the only advance which we made on Franklin's experiment, was in this, that at every step of our experiment, our electrometer enabled us to measure in terms that can always be referred to, the electrical potential of the atmosphere. Thus, for example, we first found the difference between the potential of the ground, and the potential of the air, at a point about two feet from the observatory walls and five feet above the ground. For this purpose we used the insulated water-dropper method devised by Sir William Thomson. The potential of the air at such a point, as we shall see, is almost that of the ground. With the kite 200 feet high, the aluminum pointer of the electrometer, when thrown into connection with the kite-string was deflected with a considerable impulse, beyond the limit of the scale (over 500 volts), or in other words—indicated an electrification of the air at that point, as high as 500 Daniel cells set up in series, would give. Franklin put his knuckle to the brass key and got a slight shock and a small spark. We got the same, and when our kite was 1,000 feet high, we got sparks which we measured and found to be over $\frac{1}{8}$ inch in length. We had our kite up on several days, and for as many as twelve hours at a time; and on one perfectly cloudless lovely summer afternoon, we got our first sparks from a perfectly *cloudless* sky. Aside from the scientific measurements of the potential of the air, which we were after, we found that one sitting within the "instrument-room" of the observatory, with his back to the windows, and watching only the movements of the electrometer needle could say *positively*, as quickly as an observer outside watching the kite,

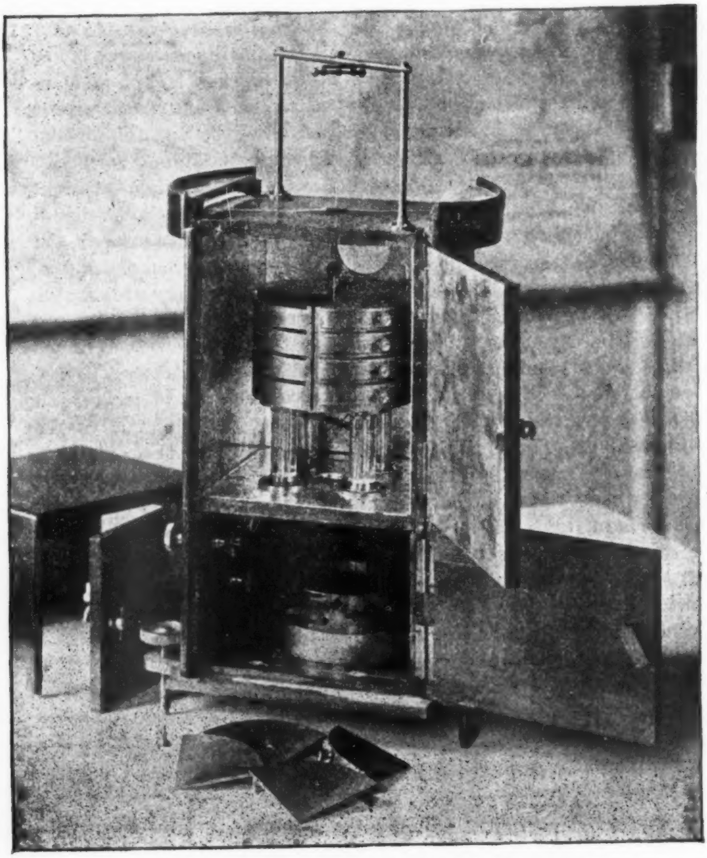


FIG. 3. MULTIPLE QUADRANT ELECTROMETER.

whether the kite was rising or falling. A rise in the position of the kite, even when 1,000 feet high, was shown instantaneously by the increased voltage and *vice versa.*

Now, to make some practical use of the knowledge thus gained, that as we ascend in the atmosphere, the electrical potential of the air increases! We must have first, some proper notion of what the rate of increase is; what, at a given elevation, the potential may be estimated at, if our purpose be to utilize this natural electrification in the service of man, or as they proudly said in by-gone days, "to harness up the electricity of the heavens,"—unaware that the electricity of the heavens is always at work in the service of man, and that every flash of lightning is visible evidence, of the expenditure of a certain amount of the accumulated electrical energy of the atmosphere, in clarifying and reinvigorating the air for man's and nature's breathing. We have a common class-room experiment of filling a half gallon fruit jar with smoke, and letting it stand a while to show that there is but little settling of the smoke particles. The cover of the jar is made of hard rubber, through which run two binding-screws and terminals. We pass a spark inside the jar, from one terminal to the other. Instantly the smoke settles, the atmosphere of the jar is cleared, the smoke particles are driven with some force to the bottom and sides of the jar—and not one or two, but frequent washings are necessary to remove the smoke odor. Our atmosphere is not a dust jar exactly, but every once and awhile it badly needs to have the dust and impurities in it settled, shaken out of it; *and this is exactly what a lightning flash does.* And a very important work it is.

Some two or three years ago, some further experiments, were made under the direction of the Chief Signal Officer, at the top of the Washington monument, Washington, D. C., which was at that time the highest edifice in the world. We were 500 feet above the city streets. Instead of the kite, we used a water-dropper collector. We will take one July afternoon. Looking out of the west window, about 2:45 P. M. far beyond Arlington and the Virginian hills, to the west and southwest, the eye saw a pallium of dark cumulo-stratus cloud creeping onward to the banks of the Potomac. The storm was possibly twenty-five miles away. It needed little experience to foretell a thunder-squall. In five minutes you can hear the rumble of the thunder; by 3:15 P. M. the storm is over head, and the lightnings are fast and vivid around the monument. Every window save the one through which the nozzle of the collector projects is closed, and the heavy marble slabs bolted, for the fury of the wind at that height and time would have driven us from our

place. But what has the electrometer needle been doing all this time! Quiet? No! From the very beginning, flying from one high value to another! 3,000 volts! and the sparks are crackling thick and fast between the fine wire we use for the suspension of our needle, and the neck of the brass top plate of the electrometer, the nearest metal to it. With all our experience and care, the harness will not hold the steed. We measured frequently as high as 3,000 volts. The sparks were popping merrily away at a rate of 10 or 15 per second. There were two intense flashes about 3:10 P. M. so remarkable as to excite comment at the time. What effect had they on us? It is an old truth that whoever seeks to pry into natural secrets, will find Nature herself willing to meet him half-way. We saw it first with these flashes, and afterwards found it to be true with every flash, that we could use our stream of water as a time-piece to *tell us when a flash of lightning was going to occur!* Previous to a flash, the stream under the influence of the increasing electrification twists and splits into threads and spray, until the very instant that the flash occurs, when it resumes its even rounded normal character. It is very much as if our jelly wall or air barrier was pulled at each end, or to make a still easier analogy, as if the air between the earth and the thunder-cloud were a rope with the cloud electrification pulling it towards itself, and the induced earth charge pulling its way. Now then, the individual fibres in the rope, as the pull increases, begin to stand out at right angles to the rope itself. Our insulated water stream, flowing from the nozzle of our collector, is an individual fibre in the rope, and can be made to give us warning of how great the electric pull is. We suggest this to all photographers who wish to know just when to expose to catch photographically the lightning flash. When the air rope breaks, the flash occurs.

As the stream of water showed itself so very sensitive to the electrification of the cloud, we were lead to wonder whether or not the charge of the thunder cloud could be so effective as to facilitate evaporation by actually pulling the water in minute drops from the stream. The following experiment seems to prove conclusively that a thunder cloud can bring about an accumulation of water and dust particles upon itself. In place of a charged cloud, we took a highly polished brass plate, well insulated, and raised to a very high potential. Bringing it near the fine stream of water, and holding it some four or five inches away, and above the stream, we find at once a deposit upon it, of minute drops of water. These have been actually torn from the stream and against gravity too.

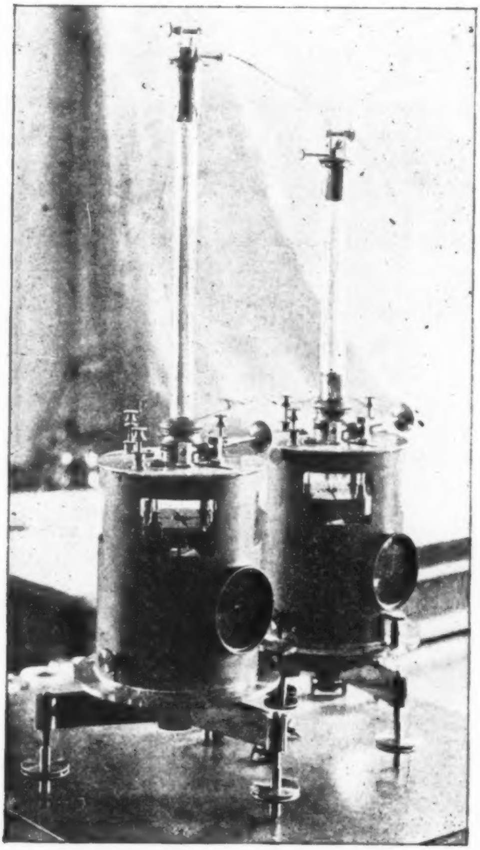
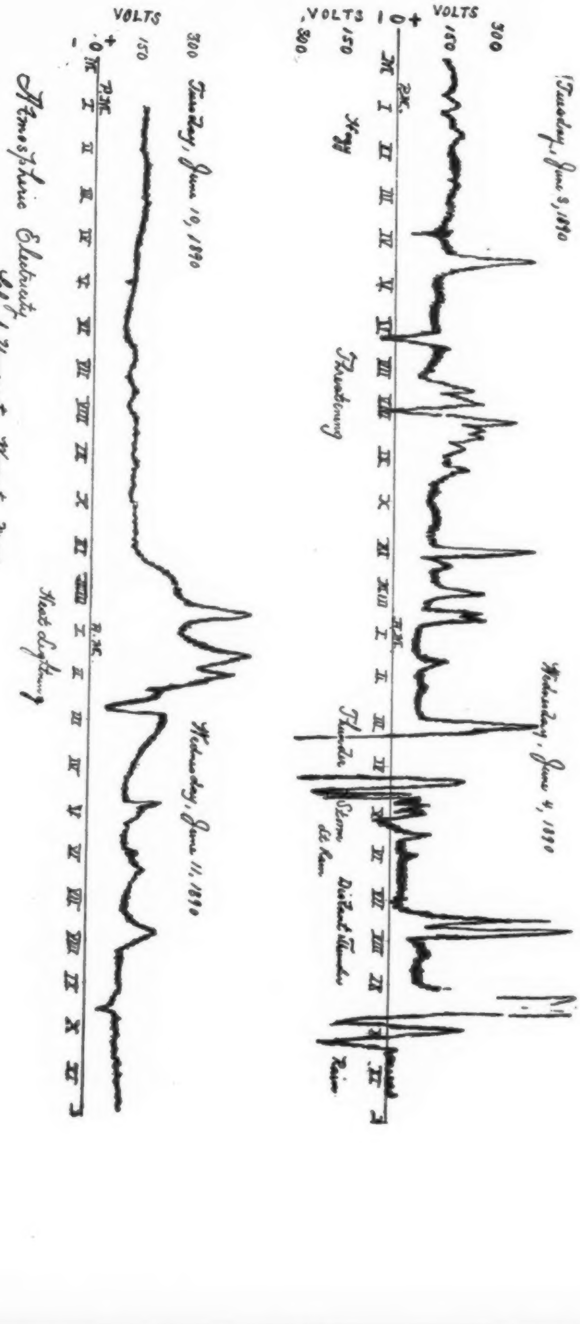


FIG 4. MASCART ELECTROMETERS.

It is now known that steam jets, as well as water jets, respond to electrification. Lord Rayleigh found it possible to close up a straggling jet; Robert Helmholtz electrified steam jets and found it possisible to get diffraction colors varying with the potential and finally Shelford Bidwell, but a year or so ago, showed both the change in color of a steam jet with electrification and the increase in the size of the water particles, and thereby the possible explanation of the orange and reddish yellow tints of the thunder cloud.

Thunder storms are, of course, the most imposing and sensational, if the word may be allowed, of the manifestations of the electrical energy of the atmosphere. But it is in the steadily continued, quiet, systematic observations of the potential that the scientist finds his field. Lightning is the show member of the group, but the auroral crown, band, streamer, dust or haze, or the St. Elmo fire, the gentle brush discharge that we find on Pike's Peak, or any high mountain, under certain meteorological conditions, is equally important. To know that we can get sparks from a sky perfectly cloudless, or that the potential of the air increases as we ascend, or that the potential gradient at a given height is determined by barometric pressure, humidity and other meteorological factors, these and many more crown the less showy work of constant observation. In the Kew Observatory, at the Parc St. Maur at Lyons, and Perpignan this work goes on, seemingly of no importance to the business world. And yet it is the unexploited work of the man of science, studying year in and year out, questions of solar physics, that determines today the ownership of land and the safety of commerce. It tells us how much the north point of the surveyor's compass changes from day to day, from year to year. Likewise, only by continuous and patient observation shall we get that knowledge, enabling us to have not only reliable bulletins of the weather, but what we may expect our bodily sufferings or enjoyments for the day to be, in so far as the weather affects these. For years many of the medical profession have wished for these curves,—the continuous records of the potential of the atmosphere in their vicinity,—for comparison with their statistics of certain nervous complaints, mental depression, hysteria, etc., etc. The horrible feelings of depression which many experience, just in advance of a thunder storm, and the great relief after the passage of the storm, if it were only possible to plot, might show perhaps a surprising correspondence with the curve of the potential changes.

Two curves obtained by photographic registers are given to illustrate the potential conditions for two days. The base line is sup-


VOLTS
Tuesday, June 3, 1890
Wednesday, June 4, 1890
Thundering
Thunder
Storm
Distant Thunder
Rain
Tuesday, June 10, 1890
Wednesday, June 11, 1890
Heat Lightning
Atmospheric Electricity
Clark University, Worcester, Mass

posed to be zero potential, or as it is commonly put, the normal potential of the ground. Beginning with 12 M. June 3, we notice a marked disturbance about 4:45 P. M., possibly due to heavy clouds, with further disturbances after culminating in the thunder storm of the early morning of June 4th. Such a day is an exceedingly trying one, especially to persons with a highly developed nervous organization. The second curve on the other hand, represents the conditions of a far less trying day. The variation in the potential due to heat lightning is noticeable.

CLOUD HEIGHTS AND VELOCITIES AT BLUE HILL OBSERVATORY.

BY H. H. CLAYTON.

As a part of the work of the Blue Hill Observatory during the last five years observations for determining cloud heights and velocities have been taken when the conditions were favorable by myself and Mr. S. P. Fergusson. In calculating the altitudes and velocities of the highest clouds, I was assisted by Mr. Arthur Kendrick who was much interested in this work.

In the case of the lower clouds the measurements were as a rule of the altitude of the base of the cloud, though on some occasions the tops of the tallest cumulous clouds were also measured.

The results are given in metric measures, first, because it is easier to compare them with results obtained in Europe, no measurements of this kind having previously been made in this country, and second, because the metric system is now becoming generally used in science and its adoption in this country is desirable and in the future probable. English readers not familiar with the metric system will be able to read this article intelligently by remembering that 1,609 meters equal one mile, and velocities in meters per second multiplied by 2¼ give approximately miles per hour.

The following four methods of measuring cloud heights have been used:

1st. The bases of the lowest clouds frequently float below the summit of Blue Hill (126 meters above the general surface of the surrounding land), and the altitude of the base can be ascertained by walking down the side of the hill.

2d. Measurements of the angular altitude of the light reflected from clouds floating over adjacent cities can be used for determing the height of the cloud.*

3d. The shadows of detached clouds can be seen from the summit of Blue Hill for many miles moving across the surface of the country and by timing the movement of the shadows between points whose distance apart is known the velocity of the cloud can be determined.

4th. Simultaneous angular measurements of the altitude and and direction of the same cloud-point have been made at two stations 1,178 meters apart with theodolites like those used at Upsala, Sweden.

An attempt has also been made to determine the height of low clouds by the difference in relative velocity between observations at the base and summit of Blue Hill, but the difference in height (126 meters) was found too short for this purpose.

The four different methods used for determining the height of the clouds to a large extent supplement one another. Thus very low clouds or strata of clouds overspreading the sky which could not be measured with theodolites are measured by the first two methods, and during gales cloud velocities and heights are sometimes measured by means of their shadows when one could not well manage a theodolite.

Accuracy of the Measurements.—The accuracy of the first method of measurement depends on the accuracy with which the base of the cloud can be determined and, as a rule, this is fairly well defined. When the summit of Blue Hill is enveloped in dense fog, one usually finds on walking down the side of the hill, that as the base of the cloud is approached, the fog grows thinner and thinner and the landscape below becomes faintly visible. A few meters farther down and one is on a level with the base of the cloud. The landscape below is plainly visible, and when the observer looks horizontally he sees the base of cloud on a level with himself and formed of innumerable ragged projections continuously changing shape and position. A hundred meters lower and he looks up to find the sky covered with an almost uniform rapidly moving stratus, occasionally, however, during rainy weather there is no well defined base of the cloud, and as one ascends the hill he merely finds the mistiness of the air growing greater and greater until finally he is

* See Amer. Met. Jour., Vol. V. page 48.

enveloped in dense fog. Twice during the last six years the writer has looked down from the top of the hill on a thin broken stratum of clouds below the summit.

The probable error of the cloud heights measured by means of the reflected light has been found by measuring the altitude of the light over various cities situated at distances varying between three and thirty miles.

For example, on June 14, 1890, at 10 P. M., the following determinations of the altitude of the base of the same cloud stratum were obtained from the measurements of the angular altitude of the reflected light: From the electric lights in Hyde Park, distant three miles, 1,500 meters; from the Quincy lights, distant six miles, 1,300 meters; from the Brockton lights, distant ten miles, 900 meters; from the Boston lights, distant ten miles, 1,300 meters; mean 1250 ± 175 meters. Several sets of measurement of this kind indicate that in general single measurements have a probable error less than 300 meters, and the mean of several a probable error less than 100 meters. By means of the light over Boston and over Providence (distant thirty miles) cloud strata as high as 5,500 meters have been measured and the results from the two did not differ from each other more than 300 meters.

The changes going on in the body of a cloud are in general so much less rapid than the horizontal movement of the cloud that the determination of the velocity of detached clouds by means of their shadows is one of the most accurate methods available for measuring the velocity of these clouds. Several measurements of the velocity of the same cloud shadow between different points rarely give values differing more than ten per cent. from the mean. But measurements of the same cloud level at intervals of ten or fifteen minutes show that variations in cloud velocity at heights below 2,000 meters are coincident with nearly equal variations in the wind velocity recorded at Blue Hill.

The cloud heights determined by measurements of the angular and the absolute velocity of the cloud are subject to an error of twenty per cent. or more, though the probable error of two or three measurements such as were usually made is probably not greater than ten per cent.

Every effort was made to insure accuracy in the measurements with theodolites. The base line was carefully measured by four students of the Institute of Technology with the most accurate appliances and has been checked by our own measurements.

The instrumental errors of our theodolites were studied and corrections applied. After the observations were made, the results were plotted with a machine made at the observatory for the purpose. With this machine the altitude and azimuth observed at each station were plotted at the same time and the lines of sight were represented by threads. When these failed to meet in a point, or to pass near each other the observation was considered poor and was discarded. About one-fifth of all the observations were in this way discarded. The heights and velocities of the clouds above the cumulus level were then calculated by Ekholm and Hagström's formulæ. (*Mesures des Hauteurs et des Mouvements des Nuages par N. Ekholm et K. L. Hagström, Upsala, 1884; Meteorologische Zeitschrift, April, 1888, page 128; Report of the Chief Signal Officer, U. S. A., Part II, 1887, page 315*). The clouds move so rapidly and are so indefinite that it is almost impossible to take observations so accurately that the lines of sight from the two stations will pass through the same point. For this reason the heights cannot be calculated accurately by a simple trigonometrical formula and the formula of Ekholm and Hagström was devised to give the distance between the lines of sight where they pass nearest each other. But calculations by this formula are so long and tedious that its use with a large number of observations is impracticable. The machine devised by Mr. Fergusson and myself would give the same result as Ekholm's and Hagström's formula if it were not for instrumental errors, but these have been made so small by Mr. Fergusson's ingenuity that for clouds below 3,000 meters the mean difference between ten carefully plotted and ten calculated heights was only one per cent. of the height and the greatest difference was only four per cent. These differences were so small that it was decided to carefully plot instead of calculate the height of clouds below 3,000 meters. Above this height the instrumental errors increase, and on this account and also because of the greater interest in the heights and velocities of the upper clouds it was decided to calculate these by Ekholm and Hagström's formulæ so as to secure the greatest accuracy possible. It is perhaps well to state that the results obtained by plotting agree much more nearly with the results of Ekholm and Hagström's formula than the results of calculation by the simple trigonometrical formula.

Ekholm and Hagström's formula admits of the mean error of each calculated height being determined; and our results

show that where two or more observations were obtained of the same cloud, the mean error rarely exceeded ten per cent. of the height, was generally under five, and will probably not average over three. Measurements with theodolites were begun in May, 1890, and are still being made.

Average Cloud Heights.—From a large number of comparisons it has been found that when the summit of Blue Hill is immersed in fog the observers at the Boston United States Weather station, ten miles north of Blue Hill record the weather as cloudy. This indicates that the base of the cloud at such times is between forty and two hundred meters above sea level. When it is foggy at Boston and over the lowlands the summit of Blue Hill is generally above the fog. On rare occasions a fog cap envelopes the summit when the surrounding sky is clear, but these caps are so thin that stars can be seen through them. Omitting these cases and counting the number of times fog prevailed at Blue Hill observatory, it can be ascertained how often the base of the cloud floated below two hundred meters. The following table shows the number of times fog was recorded at the observatory at 8 A. M., 2 P. M., and 8 P. M., during the four years 1887–90:

	8 A. M.	2 P. M.	8 P. M.
1878	51	24	41
1888	50	17	32
1889	56	21	36
1890	44	19	35
Total	201	81	144
Percentage	14	6	10

The percentage shows what proportion of all the observations were recorded foggy.

The following table gives the frequency of fog during the four years somewhat more in detail:

	—A. M.—			—P. M.—		
	7-8	8-11	11-2	2-5	5-8	8-11
Winter	49	50	39	35	34	30
Spring	63	51	30	23	38	58
Summer	58	24	14	14	21	47
Autumn	65	53	25	23	34	53
Winter half year	105	99	72	63	72	81
Summer half year	130	79	36	32	55	107
Year	235	178	108	95	127	188

With the exception of the winter the table shows that the greatest frequency of fog is near the coldest part of the day, and the least frequency near the warmest. This might be

explained either by supposing that the lower clouds are more frequent or float at a lower level at the coldest part of the day. Observation indicates that both are true. The lower clouds are frequently observed to rise above the top of the hill and slowly disappear as the warmest part of the day is approached. The winter is an exception to this in showing that the lower clouds are highest or less frequent near midnight. This is well marked in the four year totals both for January and December, and the writer has found no explanation for it.

The clouds are lowest during rainy weather and it is chiefly at such times that fog is recorded on Blue Hill. The following table shows the number of times rain was falling at 8 A. M., 2 P. M., and 8 P. M., during the four years 1887–90; and the percentages of these times fog was recorded on Blue Hill, or in other words the proportion of the time the base of the cloud was between the top of Blue Hill and Boston where it is very rarely recorded foggy at such times:

	Number of times rainy.			Percentage foggy.		
1887 90	8 A. M.	2 P. M.	8 P. M.	8 A. M.	2 P. M.	8 P. M.
Winter	52	57	62	50	33	35
Spring	59	57	62	61	39	46
Summer	52	32	32	34	28	41
Autumn	54	53	56	54	28	40
Year	207	199	218	53	33	40

This table indicates that during the coldest part of the day nimbus clouds float below 200 meters over half of the time and during the warmest part of the day about a third of the time.

The altitude of clouds higher than the summit of Blue Hill were computed from the level of Blue Hill Base station. This station is seventy-four meters above sea level and near the general height of the land surrounding Blue Hill.

In determining the average heights the day was taken as the unit. Where several observations were taken at the same hour the one having the smallest mean error was used. Otherwise the observations were given equal weight in computing the mean for the day. In computing the general average the means of the observations on each day were given equal weight.

The following table gives the height of the cumulus level of clouds determined from cloud shadow and theodolite measurements. The heights were measured by cloud shadows on thirty-seven days and by theodolites on forty-one days. The measurements by theodolites were during the summer of 1890. The

other measurements were scattered through several years. Both sets of measurements represent several hundred individual observations:

Average Heights of Cumulus and high Strato-cumulus

	Winter.	Spring.	Summer.	Autumn.	Mean.
Height in meters............	1228	1524	1528	1364	1411
Number days obs.............	5	21	38	14	

CLOUD HEIGHTS IN SUMMER.

Kind.	Blue Hill, Mass.						Upsala, Sweden.				
	Number.			Heights in Meters.			Number.		Heights in Meters.		
	Meas.	Cl'ds.	Days.	Mean.	Max.	Min.	Meas.	Cl'ds.	Mean.	Max.	Min.
Nimbus (Rain cloud) [7]	15	12	12	412	1720	20	188	125	1527	3700	2134
Low Strato-cum., or stratus [6]	22	12	8	725	1214	335	18	13	623	994	414
Cumulus (Base) [8]	113	61	28	1558	3328	601	50	36	1386	2143	730
High Strato-cum. (Fracto-cum., Flat cum., Rolled cum.) [Photo. 10]	51	22	13	1557	2752	682	165	99	2331	4324	887
"False cirrus" (Top of shower cloud) [9]	2	1	1	6500	....	....	5	4	3897	5470	2465
Alto-stratus, strato-cir., or low cir. str. [5]	5	4	4	3420	5350	2290	4	3	5198	5657	4740
Low Alto-cum., or cum. cir. [4]	20	12	7	2302	3550	820	112	76	2771	3820	1498
High Alto-cum., or cirro-cum. [3]	10	5	3	5371	7975	4772	100	56	5586	8297	4004
True Cirro-cumulus	1	1	1	....	11260	...	99	60	6465	10235	3880
Cirro-stratus, [2]	26	9	5	9652	12885	5392	56	25	9254	11391	6840
Cirrus [1]	26	11	7	10135	12351	7847	373	142	8878	13376	4970

NOTE.—The numbers in brackets refer to the number of the chromo-lithograph of each cloud in the Hildebrandson,-Köppen Cloud Atlas. The heights given for Blue Hill are the altitudes above the base of the hill, 76 meters above sea level.

The preceding table shows the average heights at which the various forms of clouds were found during the summer of 1890. These were measured entirely by theodolites except the strato-cirrus which was measured by means of reflected light, and the nimbus which was measured by its height on the side of hill or by reflected light at night. The average heights obtained by Ekholm and Hagström at Upsala are also given in this table for comparison with those obtained at Blue Hill. The number in brackets following each cloud name gives the number of the chromo-lithograph representing the cloud in the Hildebrandsson—Köppen Cloud Atlas.

The measurements by Hagström and Falk in Jemtland (*Mesures des nauges faites dans les montagnes de Jemtland pendant l'été de 1887*) agree fairly well with those made at Upsala, and indicate that the average cloud heights at Upsala represent the averages for northern Europe, as well as the small number of observations and the method of measurements permit. Except in the case of the cirrus, when the measurements in Jemtland differed considerably from those at Upsala they agreed more nearly with those at Blue Hill. Thus the average altitude found for high strato-cumulus in Jemtland was 1,788 meters which is closer to the Blue Hill average. The cirrus in Jemtland was found to be at the same height above sea level as at Upsala, and not the same height above the land at the two stations. The station at Upsala was only about 24 meters above sea level, while that in Jemtland was about 600 meters.

The only striking differences found between the averages at Blue Hill and Upsala are those for the nimbus, the high strato-cumulus, the top of shower clouds, the alto-stratus and the cirrus. The difference in the height of the nimbus was very probably due to the fact that two different types of cloud were measured. The heights at Blue Hill refer to the low ragged clouds which overspread the sky in general storms, and not easily measured with theodolites; while the measurements at Upsala probably refer to the bases of shower clouds which are much higher. The strato-cumulus was found higher at Upsala, but it is possible that the clouds included under this head were somewhat different from those at Blue Hill. The clouds included under this name at Blue Hill were mostly what Poey has called fracto-cumulus. This cloud is found to have the same height and the same average velocity of drift as the cumulus, and is no doubt merely a broken or flat form of the cumulus and differs in no

other respect. The height of the cirrus fringe at the top of shower clouds has been calculated for only one cloud at Blue Hill and for very few at Upsala, but there are several considerations that lead one to believe that the much greater height found at Blue Hill represents a real difference at the two places. The height of the alto-stratus at both places is determined from very few observations, and it is probable that a larger number of observations will show that there is no great difference in height. The cirrus and cirro-stratus both average higher at Blue Hill, and the difference is very probably a real one since the proportion of high cirrus observed was much greater at Blue Hill than at Upsala or Jemtland. In fact, in Jemtland very few cirrus were observed as high as the average found at Blue Hill.

Diurnal Change in the Height of Clouds.—The best method of determining the diurnal change in the height of clouds appeared to the writer to be as follows,—to select those days on which two sets of observations were made, one in the morning and one in the afternoon, and then to ascertain how much the afternoon heights differed from those of the morning. The only measurements numerous enough for this purpose were those of the cumulus and strato-cumulus clouds. The average diurnal change in the altitude of the bases of these clouds is shown in the following table:

Diurnal Change in the Height of Cum. and Str. Cum.

Height in Meters.		Average Change.					
8 A.–11 A.		11 A.–2 P.		2 P.–5 P.		5 P.–8 P.	
Days.		Days.		Days.		Days.	
8	0-1000	3	+ 733	5	+ 987	0	
9	1-2000	2	+ 498	5	+ 398	2	−661

All of the observations during the morning were taken between 8 and 11 A. M., and were classed under two heads, those below 1000 meters and those between 1000 and 2000 meters. Then for each class the average change in height determined separately from the observations taken at different parts of the afternoon was obtained and is given in the table together with the number of days from which the average was obtained. The table shows that the altitude of the clouds increases till the warmest part of the day and then decreases, and the change is largest for the lowest clouds. Above 3000 meters the diurnal range is probably very small. Three sets of morning and afternoon observations obtained in the cirrus region (10000 meters) gave an average height of 200 meters less for the afternoon than the morning.

The following table shows the average height of the base of the cumulus at different times of the day:

Average Height of the Base of the Cumulus.

	8-11 A. M.	11-2	2-5 P. M.
Blue Hill	1439	1777	1513
Blue Hill number clouds	24	10	31
Upsala	1082	1512	1584
Upsala number clouds	13	19	10

This table also indicates that the cumulus are highest at Blue Hill during the middle of the day; but Ekholm concludes from his observations, which are only given in a condensed form here, that at Upsala the base of the cumulus as well as the cirrus increases in height until evening. Neither of these conclusions apply to observations at Blue Hill or in Jemtland.

Velocity of Cloud Drift.—The velocities of the clouds determined from theodolite measurements have as yet only been computed from observations made during May, June, July, and 11 days of August, 1890. The averages given in the following tables are for these months.

In the first table are given the velocities in meters per second found for the various kinds of clouds.

	Low Str. Cum.	High Str. Cum.	Cum.	Low Alto-Cum.	High Alto-Cum.	Cir. Str.	Cirrus.
Height in meters	725	1557	1558	2302	5371	9652	10135
Velocity in m. p. s, Mean	6.6	8.7	8.6	9.2	16.1	33.8	36.6
" Max	14.8	19.2	11.3	13.7	39.1	51.9	59.4
" Min	2.0	1.0	2.7	4.7	3.6	15.9	19.5
Number days observation	6	12	23	7	3	5	4

The average velocity found for the cirrus (82 miles an hour) is twice as great as that found in Jemtland and Upsala. The average is not based on a large number of days observation, but it agrees remarkably well with the velocity computed by the writer from observation of relative velocity on a large number of days. (*Science*, Vol. XIII, page 246, Mar. 29, 1889). The extreme velocity (133 miles an hour) was determined from six observations and is probably correct within two meters per second.*

The next table shows the average velocity of all the clouds observed between given heights from near the earth's surface to the highest clouds. The average wind observed at Boston

* Observations taken on Aug. 11 which were plotted but not yet calculated indicated that the cirrus was moving 90 meters per second (200 miles an hour) on that day.

and Blue Hill is also given for comparison. The anemometer at the Boston station of the U. S. Weather Bureau which is ten miles north of Blue Hill is nearly on a level with the Blue Hill base station, but its height above the land immediately surrounding it is given; while the heights for the Blue Hill anemometer and the clouds are given above the Blue Hill Base station.

	Wind.		Cloud Velocities.						
	Bos.	B. H.	300 to 1000	1000 to 2000	2000 to 3000	3000 to 5000	5000 to 8000	8000 to 11000	11000 to 13000
Heights in meters......	58	126	1000	2000	3000	5000	8000	11000	13000
Number days obs......	92	92	12	28	15	4	3	3	4
Velocities in m. p. s....	4.8	7.4	7.5	7.9	9.9	13.8	36.3	31.6	37.7

This table appears to indicate that there is no appreciable increase of velocity between the top of Blue Hill and 1,500 meters higher. The writer thinks it more probable however that the wind velocity recorded on Blue Hill is too great. This conclusion is sustained by the fact that the velocities of all clouds measured below 500 meters were less than the wind velocities recorded at the same time on Blue Hill. Prof. Marvin has shown that the Signal Service form of Robinson anemometer, which is used as the standard at Blue Hill Observatory, gives velocities too great by the constant at present used, and he gives a formula for determining the true velocity (*American Meteorological Journal,* Feb. 1891, page 487). In the succeeding tables the wind velocities for Blue Hill, Boston and Mt. Washington, have all been corrected to agree with this formula. But even after correction by this formula the velocities recorded on Blue Hill were all greater than the cloud velocities observed at the same time on the three days when measurements of clouds below 500 meters were obtained. The inference is that the velocity of the wind flowing over Blue Hill is greater than that of the free air at the same height, just as water flowing over a dam is more rapid than that of the general current of the river.

Counting the observations made, both by theodolites and by following the cloud shadows, there are about 200 observations of the velocity of the cumulus level of clouds made on 82 days scattered throughout the year. This number is sufficiently large to allow a good comparison between the velocity of the clouds in the cumulus level and the wind recorded at stations of various altitudes. In the following table the average velocity of the cumulus level (including cumulus and high strato-cumulus) is given for each of the four seasons together with the averag e o

the wind velocities recorded at the time of the cloud observations at Blue Hill, Boston and Mt. Washington. The altitudes given for these stations is that of the anemometer above sea level.

Comparison of Cloud and Wind Velocities.

	Cumulus level. Height in meters.	Veloc. m. p. s.	Days obs.	Mt. Wash. altitude 1920 m.	Blue Hill altitude 202 m.	Bos. altitude 58 m.
Winter........	1228	15.1	8	21.5	12.5	7.6
Spring.........	1524	8.6	21	11.8	7.3	6.2
Summer.......	1528	7.8	30	11.0	6.1	4.4
Autumn......	1364	12.2	23	16.1	9.9	8.0
Mean..........	1411	10.9		15.1	8.9	6.5

The height of the top of Mt. Washington above the immediately surrounding country is but little if any greater than the average altitude of the cumulus clouds, but it is seen that the recorded velocity of the wind is much greater than the velocity of the clouds. On comparing the average summer velocities obtained on Mt. Washington with the table showing the velocity of the clouds at different heights it is seen that one has to rise to the height of over 3,000 meters or nearly twice the height of Mt. Washington before obtaining velocities as great as those of Mt. Washington. It appears a justifiable conclusion that the wind velocities recorded on Blue Hill, on Mt. Washington, and probably on all mountains, are too great even when corrected by the most approved formula.

The cloud velocities and the coincident wind velocities at Blue Hill summarized in the preceding table were rearanged and averaged according to the hours of the day and are given in the next table. The reason the number of days given is larger is because on many days observations were obtained both morning and afternoon.

Diurnal Period of Difference between Cumulus and Wind Velocities.

	—A. M.—		—P. M.—	
	8–11	11–2	2–5	5–8
Number days observations........	39	42	27	4
Cumulus velocities................	10.4	10.0	11.2	8.3
B. H. wind velocities..............	8.0	8.4	8.8	5.3
Difference..........................	2.4	1.6	2.4	3.0

This table shows that notwithstanding the increased altitude of the cumulus in the middle of the day the excess of its velocity over that of the wind is then at a minimum. These differences are based on a considerable number of observations and may very well be applied as corrections to the five year mean hourly wind velocities on Blue Hill for obtaining the mean

hourly velocities of the cumulus clouds. This is done in the following table:

	A. M.							P. M.				
8	9	10	11	12	1	2	3	4	5	6	7	8
Blue Hill:—												
8.0	8.1	8.4	8.4	8.5	8.7	8.7	8.8	8.8	8.5	8.4	8.3	8.4
Corrections:—												
+ 3.0	+ 2.7	+ 2.4	+ 2.1	+ 1.9	+ 1.6	+ 1.8	+ 2.1	+ 2.4	+ 2.6	+ 2.8	+ 3.0	+ 3.1
Cumulus:—												
11.0	10.8	10.8	10.5	10.4	10.3	10.5	10.9	11.2	11.1	11.2	11.3	11.5

The wind velocities at Blue Hill are for the hours ending with those given and are not corrected by Marvin's formula. The maximum velocity of the day at Blue Hill occurs at 3 P. M.; but this is near the time of minimum velocity found for the cumulus region, which appears to be about 1 P. M., and hence agrees with the results found on mountains.

The next table shows the average difference between the wind velocities on Blue Hill corrected by Marvin's formula and the cloud velocities at successive heights separated by intervals of about 200 meters. The cloud heights and velocities determined both by theodolites and shadows were used. From these differences the rate of increase of velocity per hundred meters between each successive cloud level was obtained. This is also given in the table:

Num. Days Obs.	Cloud Heights in Meters.	Differ. B. H. and C'ld Veloc.	Increase per 100 Meters.	Mean.
3	3- 500	—1.8		
5	5- 700	+0.2	+1.0	
10	7- 900	+1.6	+0.7	
8	9-1100	+3.2	+0.9	
16	11-1300	+1.8	—0.7	
7	13-1500	+1.6	—0.1	0.28
9	15-1700	+1.4	—0.1	
7	17-1900	+2.5	+0.5	
7	19-2000	+3.1	+0.3	
3	21-2300	+3.2	+0.1	
3	23-2700	+5.1	+0.5	
4	27-3000	+4.8	—0.1	
3	30-3600	+7.1	—0.4	0.28

This table shows that below 500 meters the wind velocity is less than the cloud velocity. Above that the excess of the cloud velocity increases up to 1000 meters and then decreases again till about 1700 meters, after which it steadily increases. This decrease between 1000 and 1700 meters is very probably due to the fact that the clouds between 700 and 1000 meters were

mostly observed during the morning when the cumulus move most rapidly and the clouds between 1000 and 1700 meters were mostly observed during the afternoon when the cumulus move slowest.

This causes the increase of velocity between 1000 and 1700 meters to be negative; but if the mean of the first six determinations of the increase of velocity with height between 300 and 1700 meters be taken, 0.28 per 100 meters is obtained. The mean of the next six determinations between 1700 and 3600 meters also gives 0.28. If this rate be supposed to continue uniformly up to the cirrus region and the calculated velocities be compared with the observed velocities at successive heights, the following results are obtained:

Height in meters.....	300 to 1000	1000 to 2000	2000 to 3000	3000 to 5000	5000 to 8000	8000 to 11000	11000 to 13000
Observed velocity.....	7.5	7.9	9.9	13.8	36.3	31.6	37.7
Calculated velocity...	5.7	7.9	10.7	14.9	23.2	30.3	37.3

The agreement between the observed and calculated velocities is surprisingly great considering the small number of days of observation of the upper clouds, and though the rate of increase of wind velocity with height is shown by the observations of Stevenson and Archibald near the earth's surface to be represented by a parabola, it is evident from the above results that from the height of the cumulus clouds upward it is almost a straight line, and where the heights and velocities are expressed in meters the average difference in velocity between any two levels is expressed by the formula $dv = dh \times 0.0028$. In the winter the constant probably becomes as great as 0.0033.

Special Cases.—Space will not admit of discussing the individual observations, but one or two striking phenomena seem worthy of notice. The first is the occasional reversion of the gradient at small altitudes as shown by the following observations which give the height, direction, and velocity of the clouds, and the direction, and velocity of the wind on Blue Hill, 1890; May 24, 2:54 P. M., cloud 1400 m. veloc. 3.7 from N.W, wind S.E 4.7; May 26, 2:48 P. M., cloud 1104 m. veloc. 6.9 from S. wind N.E 3.5; June 20, 8:54 A. M., cloud 830 m. veloc. 4.9 from N.W, wind N.E 5.4; August 8, 4:07 P. M., cloud 1285 m. veloc. 3.8 from W., wind E. 4.7; August 11, 3:20 P. M., cloud 1172 m. veloc. 1.7 from NW, wind N.E 3.5.

This reversal of the gradient has once or twice during six years been observed between the summit and base of Blue Hill.

In summer it can generally be traced to the sea breeze moving landward against the general gradient; and in winter to cold land air moving out to sea against the general gradient, a phenomenon as common on the New England coast in winter as the sea breeze of summer.

The following observation is of interest as tending to throw light on the mechanism of a thunder-storm: 1890, May 28, 2:55 P. M., end of cirrus-fringe projecting from the rear edge of the top of a thunder-cloud in the southwest, height 6500 m. veloc. 5.7 from N.W; 2:59 P. M., alto-cumulus following thunder-shower, height 4913 m. veloc. 21.8 from N.W; 3:14 P. M., strato-cum. southeast of zenith, height 1659, veloc. 19.2 from N.N.W; 2:59 P. M., wind veloc. 15.2 from N.W; 3:14 P. M., wind veloc. 14.2 from N.W. These measurements show that the wind velocity steadily increased from the top of Blue Hill to the height of 5000 meters where the velocity was 22 meters per second, but the rear end of the top of the thunder cloud 1500 meters higher had a velocity of only six meters per second. The writer interprets this to mean that while the main body of the thunder-cloud was moving along with approximately the velocity of the air in which it floated, namely, near 20 meters per second, the top of the cloud was flowing out from the center of the thunder-cloud with a velocity of from 14 to 18 meters per second, and hence the rear edge of the cloud only had a forward motion of about six meters per second.

Conclusion.—The writer desires to state in conclusion that these results which are the only ones of the kind available for America are rendered possible by the liberality to science of a single individual, Mr. A. Lawrence Rotch, who is maintaining the observations at much expense.

METEOROLOGICAL KITE-FLYING.

By William A. Eddy.

It is well-known that mountains operate to cool the air in their vicinity, and this fact would probably cause mountain temperatures to show marked variation when compared with temperatures taken by means of captive balloons or kites at the same altitude, as related to a valley or a surrounding country several thousand feet lower. We save time by assuming this difference without going into an exhaustive examination of the recorded

mountain temperatures in the United States and Europe, as compared with the temperatures taken by Hammon during four balloon voyages. (See the *American Meteorological Journal,* February, 1891). He discovered in one instance that the vertical thickness of a cold wave was only about 3,000 feet. During the cold wave of February 4, 1891, by means of a kite which carried a self-registering thermometer to a height of about 600 feet, a minimum of 5° lower than the surface temperature was recorded at Bergen Point, New Jersey. The cold had become marked within about two hours, and the air at the surface had only just begun to be influenced by the falling temperature or the upper air. It was an instance illustrating the fact that a cold wave could be detected promptly through either kite or balloon observations.

It is evident that we can relatively measure the vertical extension if not the breadth of a warm air current by means of self-recording minimum thermometers carried by a kite string to a great height. I have begun a series of experiments to find the approximate limit of altitude attainable through the co-operating lifting power of other kites and strings. I have found that with favorable steady winds an altitude of 1,800 feet is doubtless possible if only one kite is used. An attempt to increase the height by letting out more string may cause the kite to descend owing to the added weight, or the kite may recede and rise no higher. But if a second and larger kite be sent up until it is well within the power of the wind, its pull may lift the slacking string, increasing the altitude of the first kite by several hundred feet. The second kite—the one attached farther down the string toward the earth—if it is hexagonal will fly with a string having a very steep inclination, and so will not become entangled with the first string which may slant upward at an angle of only twenty degrees. Many hundred feet can be added to the altitude of the two kites already so very high and far off by fastening on a third and still larger kite. With string which must be of stronger material as each extra kite is used as a hoisting power, and with kites of increasing size as the earth is approached, I soon found that a third kite could be easily used to lift the others. This experiment of projecting kites into the sky with successions of concave curves in the string was continued until four kites were lifting the slack of thousands of feet of string. The general conviction of the spectators was that the fifth and highest kite was at an elevation of about four

thousand feet. This experiment was carried out at Bergen Point, New Jersey, on May 9, 1891. With the coming on of night and the increased strain upon the heavy cable-laid twine last run out, and owing to want of assistance to hold the kites, they were all drawn in with safety at sunset, and thus the experiment terminated. A number of small boys lent enthusiastic assistance, but with increased help and some simple mechanical appliances for holding the kites it would have been possible to have added a sixth kite and five or six hundred feet more to the height of all. There was a tendency in the upper kites to move to the right in accordance with the law of the upper movements at Upsala, Sweden, during a cyclone, as deduced by Ley, Loomis, and Hildebrandsson. (See *Weather*, by Abercromby, page 93). This tendency of the upper currents to move to the right was, however, more definitely shown on another occasion when a cyclone was nearer at hand, although the altitude then attained by the higher of the two kites was nothing like so great as when the five were used.

During Archibald's kite experiments in England an altitude of about 1,000 feet was attained according to an impression of Professor H. A. Hazen. It is said that Archibald fastened the same string to intermediate kites, each kite fastened to the back of the kite lower down the string leading upward from the earth. I have tried this method only twice, when I found that the lower kite was irregularly pulled upon by the upper one, causing the lower kite to dive unexpectedly, and that it is difficult to add or deduct properly from the tail of the lower kite. Perhaps with better wind and more extended experience success might be achieved, although at times the lower kite may drop suddenly, caused by the tail becoming too heavy from the slackening strain of the upper kite. But if each kite be sent up separately, then the several kites are properly balanced in accordance with a steady wind pressure. When a kite is fastened independently to the main string, with some 300 feet of string carrying it well above the veering surface currents and the housetops, then only declining or greatly increasing wind-pressure will endanger it. This element of safety applies alike to all the kites, each with its own string let out, which lift the main string leading to the highest kite.

The minimum kite temperature at Bergen Point at an altitude of about 1,500 feet (2,050 feet of string out) on February 14, 1891, was only 2° lower than at the surface (+ 28° and + 30°)

(Self-registering thermometer verified and correction given by Sergeant Jesunofsky, New York Signal Office). This small decline in temperature was due to a warm upper current. It seems possible that if we repeatedly sound upward, as it were, we may approximately estimate the vertical extension of a warm wave which may be partly or wholly nullified by the coolness of mountains. The vertical gradient at the Blue Hill Observatory was much more decided as shown by observations taken at the same time and day, for which I am indebted to Mr. A. Lawrence Rotch and Mr. H. Helm Clayton.

	4 P. M.	4.15 P. M.	4.30 P. M.	4.45 P. M.	5 P. M.	5.30 P. M.
Summit 650 feet.	14.1	14.1	13.9	13.6	12.9	12.5
Base 205 " .	17.1	16.8	16.6	15.8	15.1	14.3
Valley 60 " .	18.5	18.1	17.8	17.0	16.5	15.5

The warm upper current at Bergen Point calls attention to the truth of Mr. S. M. Ballou's deductions that the upper air in winter is warmer than the surface air, yet Hammon's balloon observations show that the air near the surface was +23.5°, but at 2,874 feet elevation it was +9.8°. At Bergen Point the air was nearly uniform in temperature to a height of 1,500 feet, but at Blue Hill we find a marked vertical gradient, illustrating the varying nature of this problem.

Since it is probable that the power of a tornado may be indicated somewhat in advance by the volume and temperature of warm air rushing northward in the southeast quadrant of a *low*, it seems that we might find in this system of sounding into the upper currents another element that would serve to advance the science of tornado prediction, emphasizing the importance of Lieutenant John P. Finley's discovery of the law of tornado distribution in the southeast quadrant.

With an extensive open prairie and with strong ropes holding canvas kites, heavy instruments can be carried to a great height with about one-twentieth the expense incurred by a captive balloon. Hexagon kites fly with several hundred feet of string extending upward at an angle of about 60°, while with a captive balloon held by a long rope a powerful wind might drive it downward. Within certain limits the stronger the wind the higher the hexagon kite flies, but the stronger the wind the greater may be the depression of a captive balloon.

CORRESPONDENCE.

SOME WESTERN GULF WEATHER.

To the Editors.—The rainfall last month (April, '91,) was-at this station, 13.84 inches. This exceeds the total precipita, tion here during any month for at least nine years. It was remarkable in that the most of it (13.32 inches) fell during three storms only, and during a brief period (17th, 20th and 21st). On April 17, 5.20 was measured in a little over two hours; 2.72 on the 20th, in a more moderate storm of four hours' duration; the most violent of the three ended this extraordinary record with 5.40 in about five hours.

During all these storms the electrical display was incessant, much lightning passing to the earth, with violent thunder. Some hail, the form and size of bullets, fell in each. Most of the rain on the days specified fell in less than half the time covered by the entire storm, and produced a general and unprecedented inundation of the region visited by these cloudbursts, which seemed nearly central to the observer, and extended in full force over some fifteen miles radius. Immediately previous there had been ten days of fresh, southeasterly wind, with heavy and increasing cloudiness and slowly falling barometer. The temperature all the while was quite uniform, the daily mean varying only 3.01 (70.04 to 73.05) in this entire period.

As is usual at this season, the upper cloud movement was from the southwest nearly, becoming more southerly toward the end, when the controlling movement was from south and south-southeast, with traces of an intermediate action from west.

Lum Woodruff.

Gallinas, Texas, May 31, 1891.

CURRENT NOTES.

The New Chief of the Weaher Bureau.—*To the Editor of the Transcript:* As a colleague of Professor Harrington, the newly appointed chief of the United States Weather Bureau in Washington, which to-day is transferred from the Army to the Agricultural Department, I send you the following bio-

graphical sketch which will no doubt be of interest to many of your readers.'

The subject of this notice is descended from the earliest settlers of New England. His great-grandfather and several others of the family of that generation took part in the Revolution, and the celebrated judge, Theophilus Harrington, was a member of this family. Through his mother, he is descended from the Dutch Walradt family, of New York. Mark Walrod Harrington was born in 1848, on a farm near Sycamore, Illinois. He prepared for college at Evanston, and graduated from the University of Michigan in 1868, when he entered the department of biological science in that institution as an instructor. In 1870, he went to Alaska as acting astronomical aid to the United States Coast Survey, in the earliest reconnaissance conducted by Mr. W. H. Dall. He returned to the University of Michigan in 1872, and, in 1876, went to Leipsic to study in that University. At the end of a few months Mr. Harrington was appointed professor of astronomy and mathematics in the School of the Chinese Foreign office at Pekin, which position he held but one year, as his health failed in that climate and he was obliged to return to America. In 1879, he was made professor of astronomy and director of the observatory at Ann Arbor, Michigan, where he succeeded Professor Watson, and this position he has held until the present time.

In 1884 he founded the AMERICAN METEOROLOGICAL JOURNAL, a scientific monthly, which has always been published at a financial sacrifice to the editors, but has nevertheless reached its seventh volume, and is the only magazine devoted to meteorology in this country. In 1886 the writer became associate editor of the JOURNAL, and three years later Dr. W J. Herdman, of Ann Arbor, joined the editorial staff.

Professor Harrington has travelled extensively, is a prolific writer, and is well versed in botanical, meteorological, astronomical and mathematical literature. He is a life member of the Linnæan Society of London, and a Fellow of the Royal Meteorological Society. That he is admirably suited for the position of Chief of the Weather Bureau is recognized by all who know him, and the writer ventures to predict that under his direction, aided by the civil character of the bureau, which

will now render enlistment in it attractive to scientific men throughout the country, the weather service of the United States will expand in directions in which it has hitherto been cramped by its military regulations, and that it will come to hold the first place among the meteorological services of the world. A general plan for the improvement of the organization of our weather service for climatic study and weather forecasts, as proposed by Professor Harrington, will be found in the recent numbers of the AMERICAN METEOROLOGICAL JOURNAL.

BLUE HILL METEOROLOGICAL OBSERVATORY, July 1, 1891.

—*A. Lawrence Rotch, in Boston Transcript.*

AN INTERNATIONAL METEOROLOGICAL CONGRESS AT MUNICH. —Mr. Scott, Secretary of the London Meteorological Office, and Dr. Wild, of the Russian Meteorological Service, having been requested by the International Meteorological Committee at its last meeting, at Zurich, in 1888, to take steps to bring about a reunion of the representatives of the various meteorological societies of the world, have issued an invitation to representatives of these services to meet at Munich, August 26, 1891. The persons invited from this country are the Chief Signal Officer, U. S. A., the director of the Harvard College Observatory, the secretary of the New England Meteorological Society, and the director of the Blue Hill Observatory. Since the transfer of the National Weather Service to the Department of Agriculture, an invitation has been extended to this department to represent the new Weather Bureau. A number of questions have already been proposed, and it is certain that their discussion, the interchange of ideas, and the resolutions adopted, will be be as beneficial to the countries taking part as were the previous Congresses held at Vienna, in 1873, and at Rome, in 1878.

A. L. R.

ERRATA.—Page 46, 8th line, for argument read *arguments*; 10th line, for part, read *path*.

CLIMATE OF THE GLACIAL AGE.—Dr. Brückner, having shown a universal change of climate of a period of thirty-five years and amplitude of about one degree centigrade (see JOURNAL, VII., 468), proceeded, in last year's meeting of Swiss naturalists, to discuss the application of the same principles to the climate of the ice-age. There were at least two distinct glacial periods and they extended over the entire earth. They could have been caused, he concludes, by a lowering of the temperature by three or four degrees centigrade. Such a lowering would cause greater storminess and greater humidity and rainfall, especially in purely continental regions. As to the cause of the lowering of the temperature, Dr. Brückner thinks it must be extra-terrestrial and periodic.

THE SAMOAN HURRICANE OF MARCH, 1889.

BY EVERETT HAYDEN, U. S. N.,

Marine Meteorologist, U. S. Hydrographic Office.

[From the PROCEEDINGS OF THE U. S. NAVAL INSTITUTE, Vol. XVII., No. 2.]

An interval of more than two years has now elapsed since the news of the great hurricane at Samoa startled the whole civilized world with its sad tidings of disaster to the American and German fleets in the harbor of Apia. The story of that terrific struggle against the fury of the northerly gale and heavy seas that swept into the unprotected anchorage; the desperate efforts of officers and men to save their vessels from collision with each other and from destruction on the sharp coral reefs; the instant annihilation of the little Eber; the grounding of the Adler and Nipsic; the breathless pause of expectation when the gallant Calliope slipped her chains, and, urging on her powerful engines with every ounce of steam that her boilers could supply, crept inch by inch "out of the jaws of death," leaving the Trenton (whose men gave her a ringing volley of cheers as she passed), Olga and Vandalia to continue their life-or-death fight against fearful odds; the wreck of these vessels and the terrible loss of life on their wave-swept decks and in the whirlpool between them and the shore; the gallantry and self-sacrifice of natives and sailors in the tremendous surf on the beach and reef—all of these

have been told and retold in the vivid words of eye-witnesses, and have already become part of the history of mankind.

It is a very different task to attempt, quietly and as time and data permit, to consider the general meteorologic conditions that preceded and accompanied the storm, and, by collecting and comparing reports from vessels and land-stations in various parts of the South Pacific, to reach at least a few definite conclusions regarding the origin and track of the hurricane, as well to derive some useful information from it regarding the weather and storms of this great ocean. It is the object of this paper to present briefly, but as clearly as the information at hand will allow, this general phase of the subject, and to publish, in advance of an official publication by the Hydrographic Office, such an outline of the facts as may serve to elicit discussion and possibly result in the collection of still more complete data, for use in the preparation of a final report. It may well be stated here, for the information of those who are not familiar with the difficulties incident to the collection of data on such a subject, that in spite of our efforts to obtain information from every possible source there are doubtless some vessels whose reports have not yet been received—reports, too, that may contain important positive or negative evidence regarding the history of the storm. Not only data from vessels, but from land-stations, also, are still wanting: for instance, the Queensland Weather Maps of Australasia and the Sydney Observatory Weather Charts of Australia and New Zealand for March, 1889, should of course be consulted, but although copies are nominally in the possession of the Signal Office, yet as a matter of fact they have been at the government bindery for six months, and at date of writing (May 9, 1891) they are still inaccessible. This should therefore be taken into consideration by any one who honors this paper by more than a mere superficial examination, and it will be interesting to note whether conclusions drawn at the present time will be appreciably modified by the missing data.

In the following discussion all dates used are east longitude dates, following the custom of the Samoan islands. Although these islands are between lon. 168° and 173° W, and might therefore be expected

to use the same dates as ourselves, yet business and other relations are so much more intimate with Australia and New Zealand that the same dates are used, as a matter of convenience. Thus, for example, at noon of Saturday, March 16, at Samoa, when the hurricane was at its height and the Calliope had just steamed out of Apia harbor, it was about 9 A. M. at Melbourne and 11 A. M. at Auckland, of the same day of the week and month, but farther east (in what we know as the *Western* Hemisphere) it was *Friday, March* 15: at San Francisco, about 3.30 P. M.; Washington, 6.30 P. M.; London, 11.30 P. M. Similarly, the first news of the hurricane, cabled from Auckland under date of Saturday, March 30, was published in Washington the morning of the same day, apparently, though really the morning of the day following.

The excitement attending the receipt of news of the disaster will long be remembered, and it is unnecessary to refer to it here further than to quote a few lines from a long statement furnished to the press, in reply to the demands of numerous reporters, by Lieut. G. L. Dyer, U. S. N., Hydrographer. The lines referred to are as follows, and they are of especial interest in this connection because, although based upon general considerations only and without any detailed information regarding this particular storm, they appear to agree very well with what actually took place:

"The hurricane that struck Samoa with such furious intensity on the 15th instant probably originated some 300 miles to the northeastward of the islands, about lat. 10° S, lon. 165° W, and moved rapidly southwestward, directly toward them. If the signs characteristic of the approach of a hurricane were observed (long feathery cirrus clouds, thickening cirrus veil, halos, and fiery tints at dawn and sunset), no doubt all possible precautions were taken to ride out the storm at anchor. The center of the hurricane, however, must have passed directly over or very near the harbor, and in the case of a very severe tropical cyclone, as this must have been, absolutely nothing can resist its fury. In the great hurricane that crossed the island of Cuba in 1844, for example, seventy-two vessels foundered at their anchors in a few hours in the landlocked harbor of Havana, a port almost unrivaled for the security of its anchorage."

The following letter from Rear-Admiral Kimberly, written only a month and a half after the storm, may well be quoted here, giving as it does a brief and concise statement of the facts as indicated by observations during the hurricane, together with such slight additional information as had been received subsequently:

APIA, SAMOA, *April* 29, 1889.

Commodore J. G. WALKER, U. S. Navy, *Chief of Bureau of Navigation.*

Sir:—The hurricane of the 15th and 16th of March at Apia was peculiar, in the fact of there being two low barometers of about equal depression, with an interval of 24 hours between. The indications preceding and accompanying the first depression gave no cause for apprehending a gale of unusual violence, and the local seamen of Apia gave it as their opinion that the weather indicated rain rather than wind, and they anticipated no destructive storm.

Friday forenoon (15th), the barometer falling, we had squalls of moderate force, and recognized the approach of the gale. The force of wind was logged 2 to 6. Steam had already been raised, and at 1 P. M., as a further precaution, lower yards were sent down and topmasts housed. At 3 P. M. the barometer commenced to rise, and it was thought the center of the storm had passed and was receding. The wind had changed from the southward to the northward and eastward in the meantime, and this fact confirmed the belief that the gale was half over. No apprehension was felt for the ships, as it was thought the latter part of the storm would be of no longer duration, and of but little, if any, greater force than the first part had developed. The barometer continued rising until nearly midnight, and it was believed that by morning the gale would be broken. There had been no very heavy sea preceding or during the gale up to this point.

At midnight, however, the barometer commenced falling again, the wind had increased, and the sea was rising high. This was the beginning of that part of the gale which accompanied the second barometric depression, and which proved so violent and destructive. The barometer continued to fall, and the gale developed its full strength rapidly. The seas also rose rapidly, and the ships felt their violence. From early morning of the 16th, for nearly 24 hours, the gale was a hurricane; and the catastrophes commenced at that time by the loss of the Eber. The story of the fate of the several ships and their crews during that day and night has been fully told, and is unnecessary to repeat here.

It will be seen that the destructive effects were due to the second depression, which followed and overlapped the first and which developed its strength so rapidly in the night. It is difficult to ascertain the exact character and move-

ments of this remarkable storm, with the unsatisfactory data afforded by the ships in the harbor, and by the meagre reports of the few vessels that were outside, which I have been able to gather.

In the future, when more data can be collected, the storm may be accurately plotted, and its peculiar features explained.

In the meantime, several theories have been advanced. It has been thought that two distinct storms passed by, following each other very closely, the second storm being the violent hurricane. Another theory is that there was but one storm, and that after passing Apia it recurved sharply to the southward and eastward, and again brought Apia within its influence.

A third hypothesis is that the hurricane was generated directly over this place, and acquired but little or no progressive movement for a long while, the rotary force as the meteor developed increasing rapidly, and causing the tremendous sea during the last half of the blow.

The unstable conditions of the storm during its formation may account for the peculiar movements of the barometer, and for its marked irregularity during the forenoon of the 16th.

I am disposed to accept this third theory; and the report that at the island of Suwaroff, 500 miles to the eastward, no gale was felt, gives it further support.

I forward a copy of the Trenton's log-book covering the period of the storm.

Very respectfully,

L. A. KIMBERLY, *Rear-Admiral, U. S. N.*,
Commanding U. S. Naval Forces on the Pacific Station.

In accordance with the plan of co-operation agreed upon between the Hydrographic Office and the Signal Office, all marine data are collected by the former office and referred to the latter, for temporary use. With the original data relating to the Samoan hurricane, referred to the Chief Signal Officer of the army, March 10, 1891, for use in preparing the Summary of International Meteorological Observations for March, 1889, a copy of a statement that I had prepared was inclosed, and the conclusions drawn therein may be quoted at some length here:

DIVISION OF MARINE METEOROLOGY,
HYDROGRAPHIC OFFICE, NAVY DEPARTMENT,
WASHINGTON, D. C., *March* 10, 1891.

Lieutenant RICHARDSON CLOVER, U. S. Navy, *Hydrographer.*

Sir:—I have the honor to report as follows upon a preliminary although somewhat complete study of all the data at hand upon the Samoan hurricane of March, 1889.

Unfortunately, certain data that ought to be available and that prove to be very essential to any correct understanding of the situation have not yet reached this office; I refer especially to detailed observations from New Caledonia and New Zealand for the month of March, as well as reports from vessels other than those from which we now have data.

To refer briefly to the leading features of the situation, I may say that the hurricane that created the destruction at Apia seems to have originated east-northeastward from the Samoan Islands, some 300 miles, on the 13th of March, probably without very great severity until the 15th, when its center passed directly over or a little to the north of Apia harbor, with a reduced barometric pressure of 29.07, wind light and variable, from 2 to 3 P. M.; at 3 P. M. the wind came out fresh from NE, shifting to north. On this date the storm commenced to recurve to the southward and southeastward, and it doubtless increased considerably in intensity during this period; to the fact that it recurved at just this position, and that during its recurve it increased in energy, must, I think, be attributed the destruction it caused in the harbor of Apia.

The only data we have regarding the earlier history of the storm are, first, the negative evidence from the statement that it was not felt at all at the little island of Suwaroff, about 550 miles E by N from Apia, and, secondly, the very brief report from the American schooner Equator, which vessel at noon of the 14th was in lat. 12° S, lon. 170° 50′ W, and experienced thick, squally weather, with winds shifting from S to SW, W and NW. The approach of the hurricane to the harbor, and in fact its general character and severity, were doubtless less clearly evident than they might have been, on account of the force of the southerly winds in its SW quadrant being lessened by the mountains on the island of Upolu. In fact, there are even now no data at hand by which to judge the actual strength of the winds in the advancing quadrants of the storm until after the 15th, nor are there any details showing the velocity of cloud movement, state of the sea off-shore, or other indications that are recognized in every ocean as characteristic of the approach of a hurricane of great severity.

After the center of the storm passed the island on the 15th and the northerly winds of its rear quadrants began to be felt, it naturally followed both that the wind itself was felt with much greater violence than the previous southerly winds (masked as they were by the hills on the island), and that very heavy northerly seas commenced to roll into the harbor. There can be no doubt but that heavier winds and seas were normally to be expected in the rear quadrant of the storm, under the particular conditions of the exposure of the harbor, but it might with equal probability have been expected that they would not be so much more severe than was indicated by the weather previously, nor of such

long duration as actually turned out to be the case, owing to the storm's recurve.

The track of the storm to the southward of the island is readily traced by means of a very good report from the American ship Hagarstown, and it seems evident that the storm was central about lat. 17° S, lon. 171° 30′ W, at local noon of the 17th, the Hagarstown being not far from the center of the storm, to the eastward. The barometric curve and the lowest reading indicated by the Hagarstown's mercurial barometer are not very unlike the curve indicated by observations at Apia during the passage of the center, although the lowest reading is not quite so low by about two-tenths of an inch; but she was doubtless at some distance from the center of the storm, which, as stated above, seems to have increased in severity during the 15th and 16th. The following day, the 18th, the hurricane passed over Nuië, or Savage Island, where great damage is reported, caused by the high winds and storm-wave, which inundated the island.

After the 19th we have as yet no very complete data by which to trace the track of the storm. The American bark Fred. P. Litchfield encountered a hurricane on the 23d, in lat. 34° 30′ S, lon. 156° W, the wind shifting from ENE to S and NW, and this may have been the same storm or it may not. Data from New Zealand, and possibly from some vessel between the Eurasia and Litchfield, might settle this question.

Farther to the SE we have no data of interest in this connection, and it is therefore impossible to prolong the track of the storm.

It is of interest to note that at the time the hurricane was raging at Apia there was another hurricane of equal or greater severity in about the same latitude but 25° of longitude to the westward. The data relating to this storm are contained in the report of the British bark Altcar, which vessel on the 16th was in lat. 16° S, lon. 161° 20′ E; at noon, G. M. T., of that day the wind was E, force 12, bar. 28.98 (mercurial, corrected), and 24 hours later wind S, force 10, bar. 29.58. The Signal Office reports from Rockhampton and Moreton, Australia, seem to show that this hurricane did not go that way, and the only data we have from New Caledonia (French transport Yarra) are too vague to draw any inference from other than that it evidently was not experienced there with any great severity. A letter from Staff-Commander R. A. Edwin, R. N. (dated Lyttelton, N. Z., July 11, 1890) states that "the weather experienced by the Altcar can be readily traced toward the East Cape"; in the absence of any complete data from New Zealand, however, I am not so sure but that the storm off the East Cape may have been the Samoan hurricane itself, which would have been felt there had it moved SSW, or even S by W, from its position on the 19th. In this case the hurricane experienced by the Litchfield

on the 23d must have been a different storm. This is a question that it seems impossible to settle without data not now available.

It will be noted from the report of H. M. S. Calliope that that vessel, when she steamed out of Apia harbor on the 16th into the northerly gale, experienced a gradual but steady rise of the barometer, as was naturally to be expected, but that on the forenoon of the 17th there was a decided fall (about .30), followed by a still more rapid rise (about .50). No such fall of the barometer is recorded in the reports from the vessels at Apia, nor do the shifts of wind help us much in accounting for it. The only hypothesis by which it can be even partially explained is that a secondary, or storm of small size but considerable severity, passed close to the Calliope and between her and the islands to the southward, affecting her barometer but not the others. There is, of course, nothing very improbable about this (although one would expect the shifts of wind to have been more marked), and the formation of this secondary, moving along a track about SE by E, may be assumed to explain the recurve to the southward and southwestward on the 18th and 19th of the Samoan hurricane itself, and its movement towards the East Cape of New Zealand (if it did move that way). Moreover, the weather experienced by the British steamship Richmond on the 20th, in lat. 18° 34′ S, lon. 153° 05′ W (wind backed to NW during the evening, blowing fresh; heavy SW sea, NW and W gale, with high sea the following day), may possibly be explained by the approach and passage of this secondary, now a storm of considerable size and severity. It can hardly be assumed to have been the hurricane encountered by the Litchfield on the 23d, however, without assigning to it a larger diameter than one would expect, or an unexpected southerly curve to its track from its position on the 20th to its position on the 23d.

An earlier hurricane that occurred during March, and whose eastern quadrants passed over the Samoan Islands, can be traced with considerable accuracy from a position at noon of the 6th, about 200 miles north of the island of Upolu, recurving W of the islands, to a position on the 8th about 150 miles E of Tonga, near which position it was encountered by the Hagarstown, which vessel experienced winds of hurricane force, and very low barometer, as indicated by her report. It is interesting to note that the heavy swell sent out on every side from this hurricane was noted on the 12th, to the southward of New Caledonia, by both the Yarra and Altcar. The log of the Trenton can be consulted for data regarding this storm, but it is of only incidental interest in connection with the Samoan hurricane.

The general and permanent interest attaching to the history of this very destructive storm renders it, in my opinion, very desirable to publish all the data that have been collected relative thereto, with as complete a discussion as possible and suitable illustration by means of maps, diagrams, and possibly

pictures illustrating the character of the harbor where this memorable catastrophe occurred. Such a publication seems called for by the efforts that we have made to collect data on the subject and the cordial co-operation that we have received from various offices and individuals. Moreover, the opportunity is an admirable one for the publication of other data of interest in this connection, that is, regarding the general subject of storms in the South Pacific. The log-books at hand in this office contain many very interesting reports, and this whole subject is one of very great interest, more especially to the commerce of our Pacific Coast. I find in the Quarterly Journal of the Meteorological Society of London a very complete account by Mr. R. L. Holmes of a severe hurricane that passed over the Fiji Islands in March, 1886, and one of the unpublished reports in this office adds very materially to the interest and value of this paper; a brief description of such a storm in the South Pacific, considered in connection with the Samoan hurricane, would be of great interest to masters of vessels.

I have the honor to request, therefore, that upon the return of these documents from the Signal Office you authorize me to complete the discussion of this storm, adding thereto such data as are available regarding the storms of the South Pacific. I beg to suggest, also, that you request the Chief Signal Officer, U. S. Army, to make an effort to obtain from the government bindery the copies of the Queensland Weather Maps of Australasia for March, 1889, and the Weather Charts of Australia and New Zealand published by the Sydney Observatory, both of which belong to the library of the Signal Office and are very essential in this connection. Very respectfully,

EVERETT HAYDEN,

Marine Meteorologist.

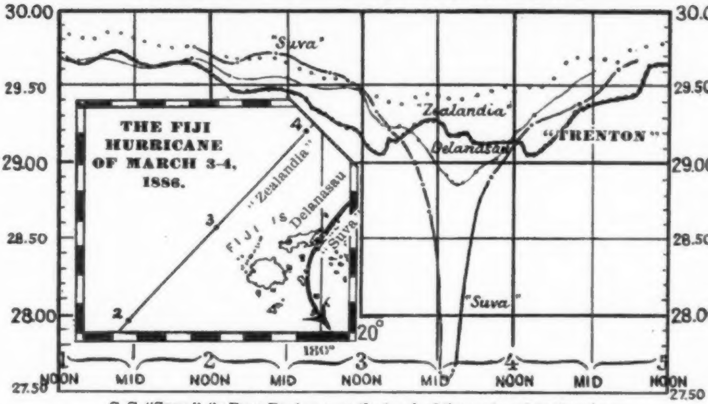
The accompanying chart illustrates graphically the tracks of three hurricanes that occurred during the month, together with the tracks of all the vessels from which reports have been received (except the French transport Calédonien, from March 13, in lat. 44° 47′ S, lon. 158° 28′ W, to March 19, lat. 50° 42′ S, lon. 130° 15′ W) and a diagram giving the barometric curves of various vessels and land-stations. Broken lines on the chart indicate absence of detailed information. The dots on the barometric curves are the data upon which they are based.

Of the three hurricanes whose tracks are charted, the first was the one that was felt with considerable severity at Apia on the 6th and 7th. It seems to have originated some 500 miles NNE from Apia

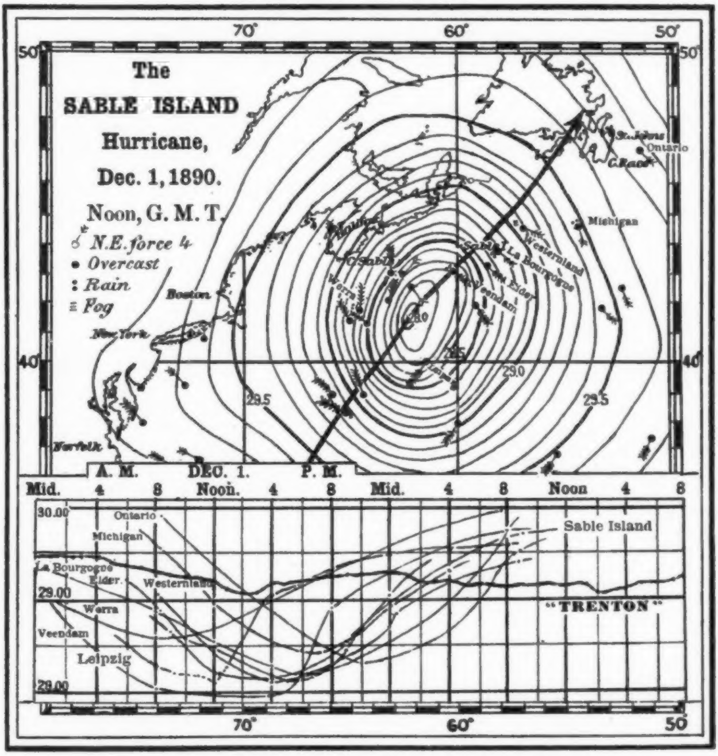
on the 5th, whence it moved in a southwesterly direction, recurving in about the latitude of the Samoan islands but 150 to 200 miles to the westward, and moving thence southeastward, between Tonga and Nuië. The barometric curve of the Hagarstown, over which vessel the center passed on the 8th, indicates that it was a hurricane of great severity—probably quite as severe as the one that succeeded it nine days later. The other tracks are those of the Samoan hurricane itself, and the very severe storm encountered by the Altcar in the Coral Sea, NW from New Caledonia.

Relative to the track of the Samoan hurricane itself, only a few words need be added to what has been said above. Probably two questions will at once occur to the reader, namely, how do you explain the two barometic depressions experienced at Apia the afternoon of the 15th and 16th, respectively (shown on the curve of the Trenton's barometer), and what caused the decided fall of the Calliope's barometer the forenoon of the 17th (this vessel, it should be remembered, steamed out of the harbor at about 10 A. M. Saturday, the 16th, and at noon of the 17th was in lat. 12° 52′ S, long. 171° 00′ W, or 60 miles NE from Apia).

Before attempting to reply to the first of these two questions, I must confess that I think there is still room for a wide difference of opinion, but I have drawn the track as seems to me most reasonable, considering the fact that we have no data from positions near Apia to the northward, southward and westward, while the conditions indicated by the data from Apia itself can certainly be explained in this way, at least quite as well as by any other hypothesis. My idea is, briefly, that the first depression occurred as the storm passed on its westward track, followed by the usual shift of wind to the northward. Along this branch of its trajectory its severity was probably not quite so great as it was later, and the force of its southerly winds was masked by the mountains on the island of Upolu; possibly careful observations of the rapidity of motion and the character of the clouds, or of the state of the sea off the harbor, might have indicated a severe storm, but this does not appear from the evidence at hand, though well worth considering. During its recurve the hurri-



S. S. "Suva" (in Buca Bay) was on the track of the center of the hurricane.
Delanasau (Vanua Levu), 80 miles from center.
S. S. "Zealandia" (at sea), 180 miles.



Barometer Diagrams from Two Typical Hurricanes,

With the record of the "Trenton's" barometer at Samoa, March 15-16, 1889.

cane probably increased in intensity, the barometric depression at the center deepening and thus causing the second depression observed at Apia, which was slightly deeper than the first although the center itself was really at a greater distance than on the previous day.

A point of interest in this connection is the fact that storms may be divided into the two following classes: First, where the barometric gradients are steepest very near the center and the wind whirls about a small central space where it is quite calm; this is the typical hurricane of the tropics, with its central "bull's eye," or calm, clear space. Second, where the central clear and comparatively calm area is very much larger, and the steepest gradients and strongest winds are found in an annular space around it, but at some distance. This distinction holds good in the case of many storms in the West Indies and the North Atlantic, and in the present instance the curve of the Hagarstown's barometer on the 8th is typical of the former class, although there is no equally good example of the latter. The second plate, however, entitled "Barometer Diagrams from Two Typical Hurricanes," illustrates the distinction very clearly by means of two examples, namely, the Fiji hurricane of March 3 and 4, 1886, and the Sable Island hurricane of December 1, 1890. The Trenton's curve is added, for comparison, and it will be seen that the indications are that the Samoan hurricane (on the 15th and 16th, at least) was of the second type, although during the 17th and 18th it doubtless became more like the first. It is interesting to note on this plate the difference between the Trenton's curve, as plotted on the two diagrams.

From amongst the various opinions that I have heard expressed by those who have studied this subject, I may be allowed to quote the following: Lieut. H. M. Witzel, U. S. N., who is thoroughly familiar with all the data, is inclined to the opinion that the second depression was caused by a storm that originated in the immediate vicinity (possibly over the island of Savaii) after the passage of the first, and remained almost stationary for some time. Mr. Arthur H. Dutton, formerly an assistant in this office, who also has studied the data

relating to this storm, thinks that from its position at noon on the 15th it recurved to the W and NW, and during the following night again recurved sharply, describing a loop north of Savaii and then returning toward Upolu, whence it moved southward and southeastward. It is thus evident that from the data at hand several hypotheses can be made that will satisfy the conditions.

As regards the decided fall of the Calliope's barometer on the 17th, we have to call to our aid, as stated above, what has been aptly termed a "convenient secondary," or local storm—a whirl within a whirl. In the absence of other information, however, I have refrained from the attempt to indicate either its origin or track.

The Altcar hurricane, as it may be called, was one of great severity, although its track, as plotted on the chart, is almost entirely hypothetic, the data at hand not indicating with any certainty whence it came or whither it went. It is of especial interest because of its relation to, or reaction upon, the Samoan hurricane, as it seems probable that its effect was to *repel* the latter and make it recurve earlier and at a sharper angle than it might otherwise have done. I am inclined to think that its true section, as it would have been given by a barometer at a land-station over which the center passed, was very different from the curve shown by the Altcar's barometer. It seems evident from her report, although it is not expressly so stated, that she ran before the wind and was compelled to remain in the storm so long that her barometric curve is deceptive, unless her action be taken into consideration and its real meaning thus explained. This hurricane may prove to have been one of those stationary cyclones that disappear near the region where they originate.

Although I have already exceeded the limits assigned, I must say a few words about the general meteorologic conditions preceding and during these three great hurricanes, likely, as they are, to be forever memorable amongst South Pacific storms. The data, if carefully studied, allow this to be done with considerable confidence, the Signal Office reports from four Australian stations supplying, to some extent, the place of the missing Australian and New Zealand weather maps.

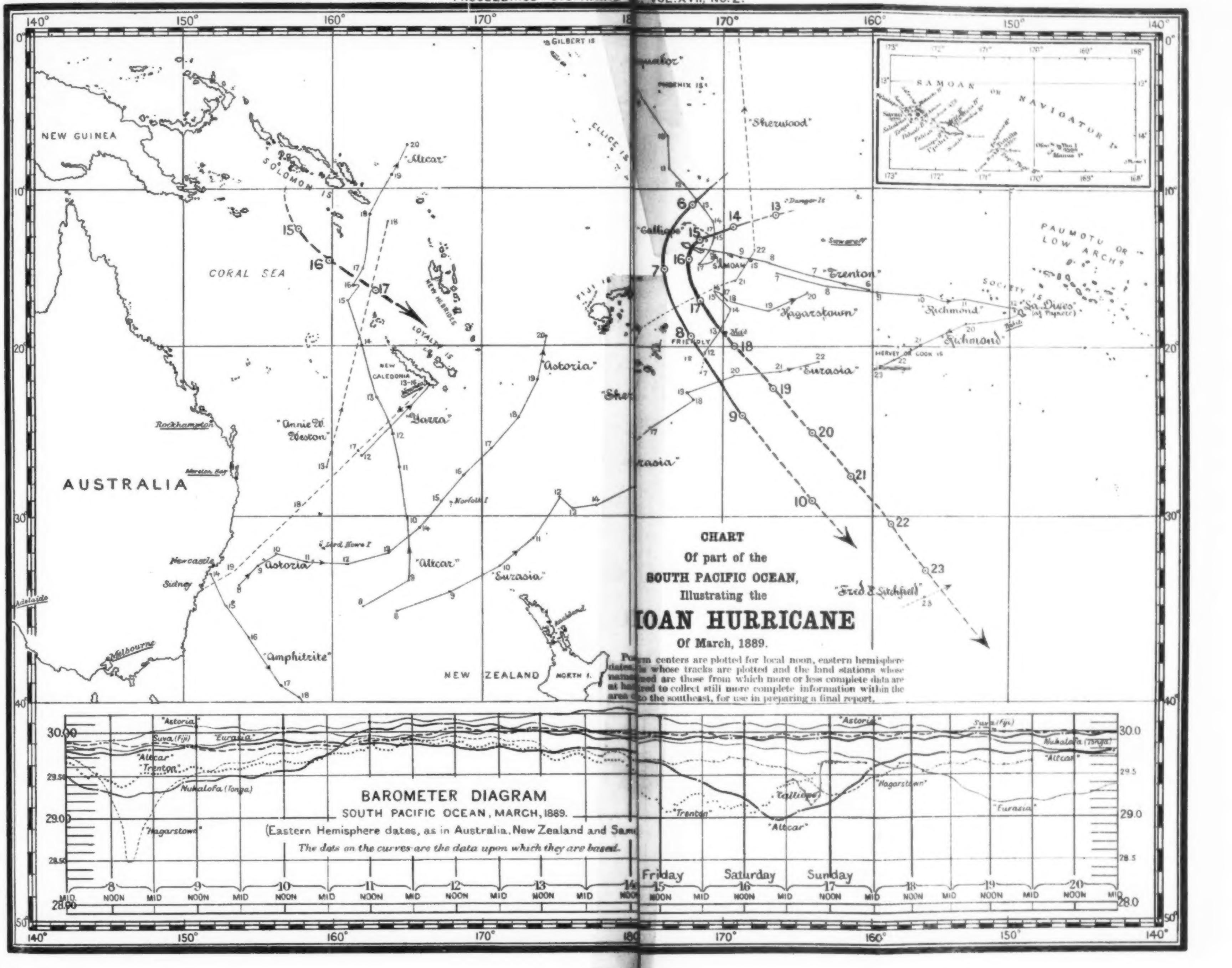


CHART
Of part of the
SOUTH PACIFIC OCEAN,
Illustrating the
AMOAN HURRICANE
Of March, 1889.
Po...m centers are plotted for local noon, eastern hemisphere
dates. ...s whose tracks are plotted and the land stations whose
name...ed are those from which more or less complete data are
at ha...red to collect still more complete information within the
area ...o the southeast, for use in preparing a final report.
BAROMETER DIAGRAM
SOUTH PACIFIC OCEAN, MARCH, 1889.
(Eastern Hemisphere dates, as in Australia, New Zealand and Sa...
The data on the curves are the data upon which they are based.
Friday
Saturday
Sunday
NOON
MID
AUSTRALIA
NEW GUINEA
SOLOMON IS
CORAL SEA
NEW HEBRIDES
LOYALTY IS
NEW CALEDONIA
NEW ZEALAND
NORTH I.
GILBERT IS
PHOENIX IS
ELLICE IS
FIJI IS
SAMOAN
FRIENDLY
HERVEY OR COOK IS
SOCIETY IS
PAUMOTU OR LOW ARCH.
SAMOAN OR NAVIGATOR IS
Rockhampton
Moreton Bay
Newcastle
Sidney
Adelaide
Melbourne
Norfolk I
Lord Howe I
Danger Is
Equator
"Alcear"
"Sherwood"
"Calliope"
"Trenton"
"Hagarstown"
"Richmond"
"La Dives"
"Eurasia"
"Astoria"
"Annie W. Weston"
"Yarra"
"Amphitrite"
"Fred E. Litchfield"
Suva (Fiji)
Nukalofa (Tonga)

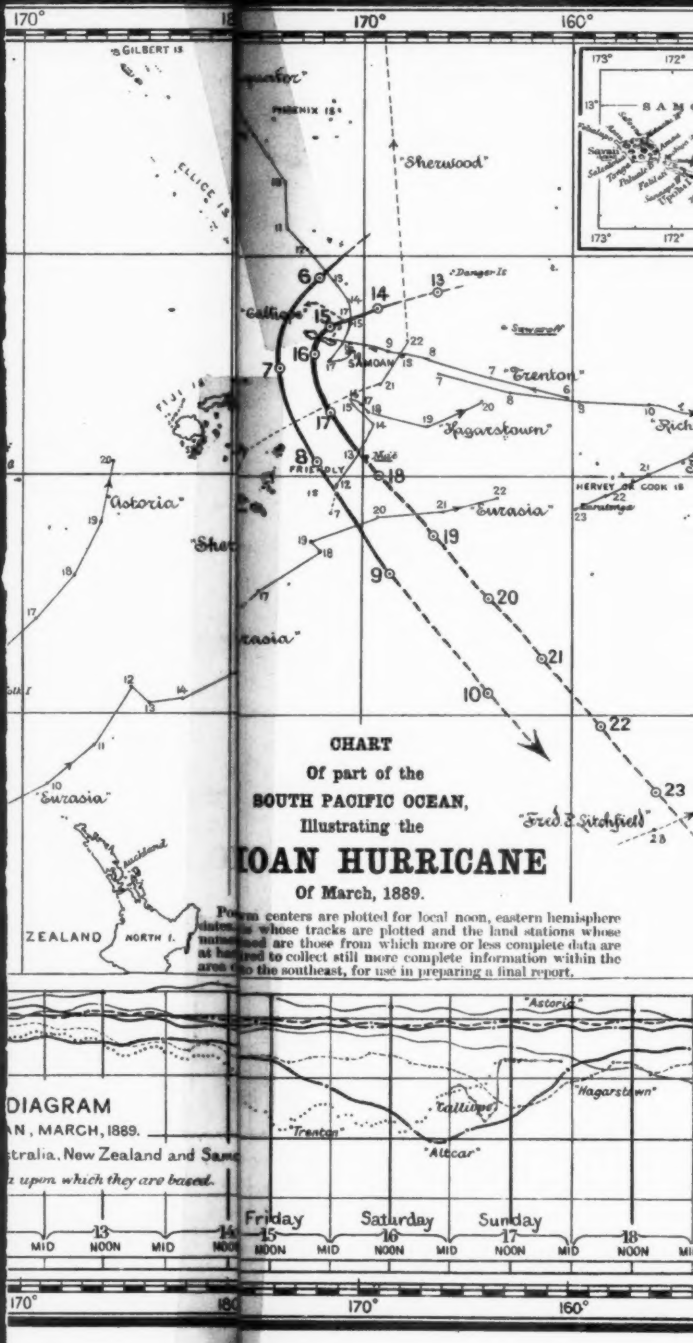
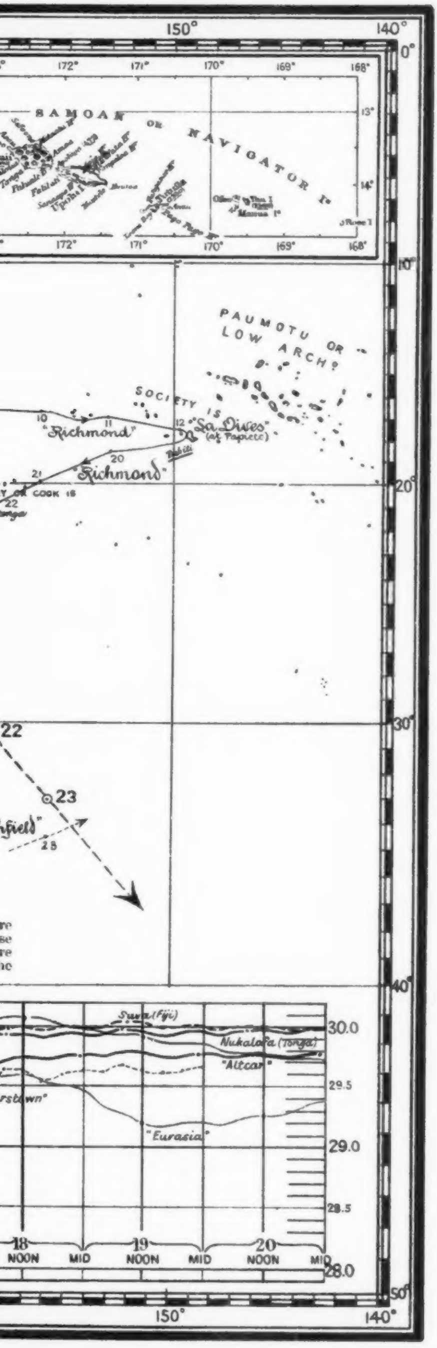


CHART
Of part of the
SOUTH PACIFIC OCEAN,
Illustrating the
IOAN HURRICANE
Of March, 1889.
Po... centers are plotted for local noon, eastern hemisphere dates... whose tracks are plotted and the land stations whose name... are those from which more or less complete data are at ha... to collect still more complete information within the area ... the southeast, for use in preparing a final report.
GILBERT IS
PHOENIX IS
ELLICE IS
FIJI IS
SAMOAN IS
FRIENDLY
HERVEY OR COOK IS
Danger Is
Suwarrow
"Sherwood"
"Calliope"
"Trenton"
"Hagarstown"
"Eurasia"
"Astoria"
"Fred. E. Sitchfield"
ZEALAND
NORTH I.
Auckland
DIAGRAM
N, MARCH, 1889.
tralia, New Zealand and Sam
upon which they are based.
"Altcar"
Friday
Saturday
Sunday
MID
NOON
170°
160°



150°
140°
172°
171°
170°
169°
168°
13°
14°
SAMOAN OR NAVIGATOR I^s
10°
PAUMOTU OR LOW ARCH^o
SOCIETY IS
"Richmond"
"La Dives" (at Papeete)
Tahiti
"Richmond"
20°
30°
22
23
28
40°
Suva (Fiji)
Nukalofa (Tonga)
"Altcar"
"Eurasia"
30.0
29.5
29.0
28.5
28.0
18
19
20
NOON
MID
50°

The normal conditions during the month of March in the South Pacific, as indicated by one of the charts accompanying Buchan's exhaustive Report on Atmospheric Circulation (published with the Results of the Challenger Expedition), are as follows. The two isobars (29.90) that inclose the equatorial belt of low pressure run nearly due east from Manila to Colon and from Central Australia to Peru, respectively. The western and wider part of the region thus inclosed has its central low area (29.75) close to the northern coast of Australia, and the isobar of 29.85 extends eastward from northern Borneo to mid-ocean (about lat. 5° S, lon. 137° W), and thence about W by S to and across Australia, passing a little to the southward of Samoa, where the normal reduced (corrected) pressure is about 29.83 during the month. Farther south, between Australia and Chile, stretches the high-pressure belt of the temperate zone, with one very decided anticyclonic system to the eastward, the isobar of 30.00 including a large oval area from the west coast of South America to lon. 140° W (pressure at center 30.25), and another similar but less decided system to the westward, where the isobar of 30.00 extends from Newcastle eastward to beyond New Zealand, and thence back over Middle Island to northern Tasmania. The southeast trades blow from this high-pressure belt toward the equatorial or low-pressure region, where, during the summer months of the Southern Hemisphere, tropical hurricanes originate, enormous whirlwinds rotating clockwise (or "with the sun," as the expression is ordinarily used) and moving gradually away from the equator along a great parabolic orbit, concave to the east, that half encircles the permanent anticyclone already referred to, west of South America. We thus see that here, as in the North Pacific and North Atlantic—in fact, as in every ocean—it is the western portion that is most subject to hurricanes, and they rarely occur farther east. To the southward of this high-pressure belt of the temperate zone, toward and perhaps to the South Pole itself, pressures decrease very rapidly and uniformly, the isobar of 29.30 coinciding almost exactly with the 60th parallel. This is the region of almost continuous westerly gales, varied by an occasional storm or hurricane. The normal or average

conditions are, of course, greatly modified occasionally by disturbances which, although not of frequent occurrence in the tropics even in summer, are sometimes very severe.

The conditions during the early part of March, 1889, seem to have been about normal up to the 4th, when British Meteorological Office reports from Suva, Fiji, and Nukalofa, Tonga, indicate that an anticyclone extended southward toward New Zealand. As this system moved slowly eastward and a cyclonic storm passed southeastward along the south coast of Australia and Tasmania, the first of the three hurricanes described above formed north of the Samoan islands and an apparently feeble depression developed over the Coral Sea. This last depression disappeared as the hurricane moved south of Samoa on the 7th and 8th, and a strong anticyclone appeared over South Australia and moved slowly to the southward and eastward with apparently increasing intensity, becoming central on the 13th in the vicinity of Tasmania, with corrected barometric pressure as high as 30.47 at Melbourne.

It was on the 12th that the very earliest signs of the hurricane's approach were observed at Samoa. To quote from notes made by Lieutenant R. G. Davenport, U. S. Navy, the navigator of the Nipsic, "there was a peculiar, coppery-red sunset the evening of the 12th and the weather was clear the first part of the 13th, but overcast toward evening, when the barometer stood .21 below its reading the preceding day. Calm and light southerly breezes prevailed, force 0 to 2."

On the 14th the weather grew still more threatening and the barometer continued its steady fall, now slowly, as the time of the daily maximum approached, and now more rapidly, as the fall due to the influence of the approaching storm combined with the daily ebb of the barometric tide (always such a marked phenomenon in the tropics). Toward evening the ships got up steam in their boilers, that their engines might aid their anchors in keeping them off the reefs and preventing collisions with other vessels in the crowded harbor. It was doubtless an anxious moment for the commanders of the naval forces of the three great nations, responsible, as they

were, not only for lives and ships but for the prompt execution of their instructions and the faithful guardianship of public interests committed to their care. To most of the others on board, both officers and men, free from at least some of the cares and responsibilities of their superiors, the actual danger of the situation was probably not fully evident till after the shift of wind to the northward Friday evening, when the long battle with the elements commenced in earnest.

But to resume and to conclude: Whilst the hurricane was approaching Samoa on the 15th the Tasmanian anticyclone had moved toward New Zealand and the Altcar hurricane had probably already formed in the Coral Sea. On the 16th both hurricanes were raging with terrific intensity, and the Samoan, recurving and almost doubling on its tracks, was playing havoc in the harbor of Apia. It was on this day, Saturday, that the greatest destruction occurred, and it was this and the following day that saw those scenes of heroism, self-sacrifice and devotion that for months made the wreck-strewn ledges and beaches of Apia harbor the focus of public attention and that must for centuries elicit the praise and admiration of mankind.

THE ANNUAL OF THE OBSERVATORY OF LA PLATA* for 1891 is the fifth in the series, and is like its predecessors, except that it is larger and its tables more extended. The city of La Plata is a modern and rapidly growing one, a few miles southeast of the city of Buenos-Ayres. Sr. F. Boeuf is director of the observatory, which promises to be an active rival of the older and better known one at Cordoba. The *Anuario* contains tables of various sorts, something after the plan of the *Annuaire* of the Bureau of Longitudes in Paris. The meteorological tables occupy pages 283 to 369, of which a few pages only are devoted to the results of observations at La Plata. An elaborate meteorological system is planned for the province of Buenos-Ayres, consisting of 13 supported by the province and 20 by the railroads.

* Anuario del Observatorio de La Plata para el Año 1891. Buenos-Aires, 1891. 12mo, 505 pp.

BOOK NOTICES.

Deductive Methods of Weather Prediction.*

The objects of this paper, as stated by the author, are to consider the physical principles involved in the formation and motion of storms, and to give a deduction or philosophical method for their prediction. This is attempted without the use of mathematical analysis. The author hopes that the methods of meteorology may become hereafter comparable in accuracy and elegance to the analysis used in the exact sciences. Meteorology is looked on as strictly an application of hydrodynamics and thermodynamics to the atmosphere, and the attempt is made to sketch out the character which the treatment must hereafter show, and to get, by graphical and other methods, some quantitative idea of the elements involved. The influences of lunar, stellar, internal and meteoric heat, of variations of solar radiation, of atmospheric electricity, of the tides, and of terrestrial magnetism are dismissed as insignificant; the rotation of the earth, gravitation and solar heat are taken as the primary sources of energy, and the absorption and radiation of heat by earth, clouds and air, and the friction of earth and air, and the changes of heat in evaporation and condensation of water are the only processes considered by which heat is transformed and transferred. Friction receives some discussion, and considerable space is given to cases of aerial vortex motion. Turbulent flow, buoyant ascending motions, the horizontal motions and buoyancy of clouds, rain, and the mechanism of storms are the other topics considered. The whole forms a mass of interesting matter, full of suggestion and deserving the study of meteorologists. It was apparently written some years ago, and some of the more modern ideas could not be incorporated or discussed. This is one of the necessary misfortunes in treating of even the larger features of so rapidly changing a science as meteorology.

The proof for this report was apparently read during the absence of the author, and, it appears, at the last moment some changes in it were rendered necessary. The consequence is that 53 cuts which are referred to in the text appear nowhere in the report itself; there are also unevennesses in punctuation and the construction of sentences.

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* Preparatory Studies for Deductive Methods in Storm and Weather Predictions, by Professor Cleveland Abbe, LL. D. Appendix 15 of the Annual Report of the Chief Signal Officer for 1889. Bound separately. Washington, 1890. 8vo, 165 pp.